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UNITED STATES DISTRICT COURT
EASTERN DISTRICT OF NEW YORK

ROBERT A. FALISE, LOUIS KLEIN, CHRSTIAN E. MARKEY, JR. as	)	JR., FRANK MACCHIAROLA, Trustees,	)
	)	Plaintiffs,	)
	)	vs.	) No. 99 CV 7392
	)	(JBW)THE AMERICAN TOBACCO COMPANY,	)
et al.,	)		)
Defendants.	)		)

CONTINUED VIDEOTAPED
DEPOSITION OF DAVID M. BURNS, M.D.
San Diego, California
Friday, June 9, 2000
Volume 2

Reported by:
RENEE KELCHCSR No. 5063
Job No. 109049

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	)	vs.	) No. 99 CV 7392
	)	(JBW)THE AMERICAN TOBACCO COMPANY,	)
	)	Defendants.	)

Continued videotaped deposition of DAVID M. BURNS, M.D., Volume 2, taken on behalf of Defendants, at 1515 Hotel Circle South, San Diego, California, beginning at 9:13 a.m. and ending at 5:36 p.m. on Friday, June 9, 2000, before RENEE KELCH, Certified Shorthand Reporter No. 5063.

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San Diego, California, Friday, June 9, 2000
9:13 a.m. - 5:36 p.m.

DAVID M. BURNS, M.D.,
having been previously duly sworn, was examined and testified as follows:

MR. GRUENLOH: Mike Gruenloh, Ness Motley on behalf of plaintiff.
MR. BERNICK: David Bernick for Brown & Williamson.
MR. SCHROEDER: Tom Schroeder for R.J. Reynolds.
MR. STEIN: Adam Stein for B.A.T. Industries, PLC.
MR. PARSEGHIAN: Berj Parseghian for Philip Morris, Inc.
MR. THOMPSON: Brent Thompson for R.J. Reynolds.
MR. GRUENLOH: Mr. Bernick, at the end of the deposition yesterday you said you were going to attempt to get the judge -- or, I'm sorry, Magistrate Gold on the line this morning. What's going on with that? End the suspense.
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MR. BERNICK: What I've done is to contact Judge Gold's chambers and determine his schedule for today -- he always tells me, and I've now seen, that he's a very busy guy -- to see if there's time when he can take up problems that we have with the deposition. I'm told that he's available between 2:00 and 3:00 today, which as I do my arithmetic is between 11:00 and 12:00 this morning.
MR. GRUENLOH: Okay.
MR. BERNICK: So depending upon how things go, we'll see if we need to do that, my overwhelming preference is not to do that. But I just can't tell how the deposition is going to go. So that's the contact I've had with the court's chambers. I've not spoken with Judge Gold, himself.
MR. GRUENLOH: Okay, thank you.
EXAMINATION (Continued)
BY MR. BERNICK:
Q Good morning, Dr. Burns.
A Good morning.
Q In your report -- do you have your report here in front of you? I don't know if that was marked as an exhibit.
A I assume that it's probably in this list.
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Q Yes. I think Exhibit -- must be Exhibit Number 2.
A 2 is what I have.
Q I'd like you to take a look at paragraph 10 of your expert report in this case.
Does paragraph 10 of your report set out your views as an expert on how scientific consensus is reached --
A Yes.
Q -- on the issue of whether smoking causes a given disease?
A Yes.
Q And as I understand what you're saying in paragraph 10, is that reaching a scientific consensus involves a process; is that correct?
A There are several ways that the term "consensus" is used. There is a formal consensus process, such as the ones that are used to generate the Surgeon General's report. There is also the English language use of that term, which is that the majority of the scientific community has reached a conclusion, or knowledgeable scientific community has reached a conclusion on a given issue.
Q I'd like -- my question, I think asked you --
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I'd like to focus on the views that you have set forth in paragraph 10 of your report about how a scientific consensus is reached. And my question to you in particular is whether those views include the view that a process must take place?
A As I just described, I used the term in two contexts. One is the formal process of reaching a consensus such as that used by the Surgeon General's report. And the other is the way that the term is used as an English language term for the preponderant opinion

for the scientific community. One, the first, requires a process. The second is something that is a state of the art.

Q Is there anywhere in your report where you set out any statement concerning how scientific consensus is reached other than in paragraph 10?

A I don't believe so.

Q Focusing on paragraph 10, as you have described how a scientific consensus is reached in paragraph 10, does that involve a process?

MR. GRUENLOH: Objection; asked and answered.

THE WITNESS: I have answered that question. The use of the term in that paragraph is intended to reflect a process used by the Surgeon General's report

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which is, indeed, a process.

BY MR. BERNICK:

Q What paragraph 10 talks about is that a process is performed by a set of expert reviews of the data; correct?

A It describes the Surgeon General's process for formation of scientific consensus, which is one of preparations of drafts, review of those drafts, revision, re-submission for review, again revision, and then submission to the Public Health Service for review, and ultimately for clearance.

Q Would it be fair to say that the statements that you make in paragraph 10, the description in paragraph 10 of your report relating to how consensus is formed, is designed and only does cover what's taking place in connection with the Surgeon General's reports?

A It is intended to discuss the Surgeon General's report process for forming consensus, that's correct.

Q And does paragraph 10 fairly, in your view, describe the process that has, in fact, been followed in the course of issuing the Surgeon General reports?

A I believe so, yes.

Q The -- you've referred to a consensus. Consensus, as used in the English language, and as I

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understand your testimony, you believe that there are other ways that scientific consensus can be reached other than as described in paragraph 10; correct?

A There are a variety of other processes, of formal consensus formation. One of which was largely originated in 1970s, which is called formally a consensus process. That has been widely used at NIH on various scientific and clinical topics.

Q So --

A There is also the -- a variety of other approaches, such as that used by the National Academy of Sciences and a variety of other organizations to achieve a similar outcome to the Surgeon General's report. Those are more or less formalized, depending on the organization. And then there is the general process of scientific publication and review that leads people to progress in their understanding of an issue scientifically.

Q The only process for reaching scientific consensus that you describe in your report is the process followed by the Surgeon General; correct?

A The process I described was one in this paragraph, was the process used by the Surgeon General. I refer to consensus elsewhere in the report, I believe,
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as to the English language use of the term as scientists reading literature and reaching conclusions from it.
Q If you'd focus on the question. The question asks for whether you have described the scientific consensus process anywhere else in your report other than in paragraph 10?
MR. GRUENLOH: Objection; asked and answered.
BY MR. BERNICK:
Q I don't think we're communicating, Dr. Burns. Paragraph 10 describes how scientific consensus is reached, does it not, in conjunction with Surgeon General reports?
A Paragraph 10 describes a consensus formation process; that process used by the Surgeon General's report.
Q Now I'm asking, is there any other place in your report where you describe how scientific consensus is reached?
A There's no other place in the report where I describe a formal process for achieving scientific consensus.
Q Is there anywhere else in the report where you set out the guidelines that are followed in reaching scientific consensus?
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A I don't believe so.
Q Are there any other guidelines that you state anywhere else other than your report -- strike that.
Are there any guidelines that you describe anywhere in your report for reaching scientific consensus other than the guidelines that are set out in paragraph 10?
A Well, paragraph 10 does not set out guidelines. Paragraph 10 sets out a process that was used. It describes the process the Surgeon General's report used. It is not a set of guidelines or technical criteria for consensus formation. It simply describes the process that was used.
Q Okay. Is there -- are there any guidelines or standards for determining when consensus is reached on the issue of smoking and health?
A There are multiple sets of those guidelines, including ones that have been developed by various organizations tasked with developing guidelines. There are various groups that have internal processes for examining this data. This data and other data. The Surgeon General's process has a specific approach to this, and much of that approach was conditioned by the criteria defined in the 1964 report.
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Q Okay. The criteria defined in the 1964 report are criteria for establishing causation?
A They are -- there is a discussion of how one draws causal attribution from a variety of data on a given topic. And there's a detailed discussion of both how that is done and the use of an examination of

epidemiologic data in that context.
Q If you focus on my question. You said the word "criteria." I simply asked you, are there criteria that are set out in the 1964 Surgeon General's report on making a judgment on whether causation has been shown?
A I'm -- I guess I'm confused. I thought I answered your question. If you want me to take another shot at it, if you want to try and clear it up.
Q No. I want you to tell me, yes or no, are there criteria set out in the 1964 Surgeon General's report for determining whether causation has been shown?
A The 1964 report discusses the basis under which a judgment of causality can be reached, and then lays out a set of criteria for reaching a judgment. In particular, it focuses on the kinds of criteria that can be used for evaluation of epidemiological data in examining the question of whether causality has been established.

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MR. SCHROEDER: Would you mark that one, please? Thank you.
BY MR. BERNICK:
Q The criteria that were used and described in the 1964 report, are those criteria also set out in the paragraph 10 of your report?
A They're -- the terms that are used in paragraph 10 are similar to the headings for each of the discussions of criteria for the examination of epidemiological data and causality. They are not the sum total of that discussion within the Surgeon General's report, but they are the terms that are used for examination of epidemiological data in the context of establishing causality.
Q But just to be plain, there were five basic criteria for making a judgment about causality in the '64 report; correct?
A We've now been through this three times. You --
Q Please answer the question, Dr. Burns. Are there five criteria or not in the '64 report?
MR. GRUENLOH: So I don't have to object every time, I would like to place an ongoing objection for every time Mr. Bernick asks the question multiple times.

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Go ahead and answer the question, Dr. Burns.
THE WITNESS: I've tried to make this clear several times now. And I don't understand where the confusion --
BY MR. BERNICK:
Q There's a question before you, Dr. Burns. Are there five criteria or not in the '64 report?
A The 1964 report lays out a set of approaches for examining whether data can establish that a given agent, or exposure to a given agent causes a given disease. Within that context -- within the context of that discussion there is a detailed description of how epidemiological data can be used in that process. Within that there are five criteria that are laid out and discussed in some detail. The headings of those five criteria are the ones here in paragraph 10.
If I've not been clear in what I've said, and

it doesn't answer your question, please help me as to where I'm not being clear and where that question is not being answered, because I don't understand what more I can tell you.

MR. SCHROEDER: Mark that.

BY MR. BERNICK:

Q Did the Surgeon General in 1964 make a

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determination that based upon those five criteria, the epidemiological evidence, together with other evidence, was sufficient to make a judgment that smoking caused disease?

A I think you are mischaracterizing the description in the Surgeon General's report.

Q Dr. Burns -- that's fine.

A Okay.

Q Go ahead.

A The Surgeon General's report states very clearly that one makes a judgment about causality from assembling all of the information and examining it. They then go on to say that epidemiologic data is very important. And in order to examine epidemiological data in the context of all of the data, you use these criteria.

Q Are you aware of any statement prior to 1964 of a scientific consensus on the issue of whether smoking causes any disease?

A Yes.

Q I want you to identify any statement prior to 1964 which sets out a scientific consensus of the scientific community on whether smoking causes disease.

A There are several with increasing degrees of

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formality in terms of the process of consensus. Early on there was an editorial in the New England Journal. That is an individualized expression of what that person believes to be the consensus. Dr. Burney's statement in the mid to late 1950s was done as the official act of the Surgeon General after reflection on the data, which is another form of consensus.

In 1962 there was a formal process conducted by the World College of Physicians, which examined this data and reached a formal judgment through a process of examining all the data, that cigarette smoking causes disease. Each of those can be described variably as a consensus with increasing degrees of formality in the process that they underwent to achieve the consensus statement that came out.

Q When did the editorial come out of the New England Journal of Medicine?

A I don't have a specific date from memory. My recollection is that it was in mid 1950s; '55 or '56.

Q Dr. Burney's statement, when was that?

A That was in the late 1950s. I believe it was 1959, but it could have been '58 or '57.

Q And that was the statement that appeared in a publication, the Journal of the American Medical

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Association?

A You would have to show me it. He made a number

of publications during that time.
Q Does this refresh your recollection?
MR. GRUENLOH: Do you have another copy of that?
MR. BERNICK: No.
MR. GRUENLOH: Are there going to be multiple documents that you don't have a copy of that you're going to be showing the witness today?
MR. BERNICK: Maybe yes, maybe no. It depends.
MR. GRUENLOH: As a courtesy, could you get another copy of those, so I can review them?
MR. BERNICK: I don't have time to make all those different copies. I don't think it will impair the examination.
I'm just asking whether that refreshes his recollection that the statement that he said that Dr. Burney made, was made in 1959 in JAMA.
THE WITNESS: I believe this is it, yes.
BY MR. SCHROEDER:
Q The New England editorial was authored by whom?
A I don't recall the author.
Q Prior to the 1962 Royal College report, are you
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aware of any consensus statements of the scientific community on the causation issue that involved the performance of a set of expert reviews?
A Other than the ones that I have outlined thus far, I'm not specifically aware of credible reviews that were conducted prior to then.
Q Okay. But in the ones that you've described so far, the editorial in the New England Journal, did that involve a set of expert reviews?
A The New England editorial by definition is thought be to be an expert opinion.
Q I didn't ask you that. I said --
A Well --
Q -- did the editorial in the New England Journal involve the performance of a set of expert reviews?
A Perhaps you could define for me what you mean by "set of expert reviews."
Q Well, you used the words "set of expert reviews" in your expert report.
A I understand that.
Q All I'm asking you, Dr. Burns --
A I understand that, but you're telling me that every time I try to use it as --
Q Excuse me. I try to not to interrupt you. I
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know that's very hard for me.
A Go ahead.
Q Could I have the same courtesy? However you use the word -- or words "set of expert reviews" in your expert report, Dr. Burns, did the editorial in the New England Journal involve a set of expert reviews?
MR. GRUENLOH: Object to form.
THE WITNESS: As I have made plain now innumerable occasions, that terminology in my report describes the Surgeon General report's process. The first report that used that process was the 1964 report. Okay? Therefore --
BY MR. BERNICK:

Q I understand --
A -- by definition there was no report prior to the first one which was released in 1964.
Q My question for you, understanding that you were intending to describe in your report the Surgeon General reviews --
A Yes.
Q -- the Surgeon General reports, I'm simply asking about whether some of the characteristics that you pick out for that expert review, for that consensus process, were in existence prior to 1964. And the first

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one that I've picked out is "set of expert reviews."
So my question to you is very simple. Did the editorial that appeared in the New England Journal that you had reference to, did that involve the performance of a set of expert reviews?
A I would assume that it did. Okay?
Q Do you know that it did?
A Please let me finish my answer.
Q Sure.
A Okay? The normal process by which an editorial appears in the New England Journal is that the editors of that journal ask someone to write it. It is then reviewed by an external set of experts, and then it is published. That is the process by which an editorial appears. Therefore, it would have undergone a set, more than one, of expert reviews of its content. Is that a process comparable to the Surgeon General's process, with all its detailed checks and balances? No, it is not. It's a much more limited process, but it is one that is a process.
Q Did the set -- did the editorial that appeared in the New England Journal use or apply the criteria for assessing epidemiological evidence that are referenced in your expert report?

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A Not explicitly. They used the criteria used in the Surgeon General's report of defining judgment based on an integration of all of the data that existed. They did not formally apply the specific criteria in 1955 that had not been developed prior to the 1964 report in its form as described in paragraph 10.
Q Are you saying that the criteria for assessing epidemiological evidence that appear by name in paragraph 10 in the 1964 report had not been developed prior to the 1964 report?
A No. No. I'm saying that the 1964 report formalized them in a very specific way. They are drawn from prior considerations, particularly the Bradford Hill criteria, among others, that had been discussed in the literature prior to that time.
Q Well, then let me come back to my question. Do you know the editorial that appeared in New England Journal actually applied the criteria for assessing epidemiological evidence as those criteria are referenced in your expert report?
A The criteria that are referenced in my expert report first appeared in 1964 in the Surgeon General's report. There was a scientific and philosophical discussion about the termination of causality that

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antedated that in the scientific literature, including the development of criteria by epidemiologists such as Bradford Hill.
Q I'm just asking you, do you know what criteria were applied to the epidemiological data in the editorial in the New England Journal?
A I am trying to answer your question.
Q Tell me what criteria were actually applied in the assessment of causation that appeared in the New England Journal. Do you know?
A The criteria of integrating scientific information to form a reasoned judgment.
Q Is that it?
A There were no formal criteria laid out in that editorial. You're asking -- I mean, I don't understand what -- what you're doing. You're asking me for formal criteria in an article that doesn't have formal criteria laid out. You're then asking me what criteria are used. When I give you the criteria that are used, you don't want to accept those. I don't know where to go with this.
MR. SCHROEDER: Mark that.
BY MR. BERNICK:
Q Well, are you or you not saying that there were
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formal criteria for causation that were set out in the editorial that appeared in New England Journal? Yes or no?
A I'm saying that the person who wrote the editorial in the New England Journal was, indeed, a scientist who was familiar with the process scientifically of drawing causal inference from data. That individual did not feel compelled to write down as explicit criteria the criteria that they were using in the formation of that judgment. And therefore, that component of the process that they used was not included in the text of that editorial. That is not to say that that individual was not trained in and did not use a scientific process for assembling data to form a judgement about whether cigarette smoking caused lung cancer.
Q Dr. Burney's official statement that you say was made in the 1959 JAMA article, did that involve the performance of a set of expert reviews?
A It is my understanding that that article was generated within the Public Health Service, within the office of the Surgeon General, and in that setting would have undergone a substantial number of expert reviews prior to its submission to the journal. The journal
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would also have subjected it to a set of expert reviews.
Q Is that the -- is that the kind of expert review that took place in connection with the '64 report, or is that a different kind of expert review?
A That is the kind of expert report -- expert review that occurred in conjunction with the preparation of an official position of the Public Health Service. That is similar to, but nowhere near as extensive, as the process that occurred in '64 and subsequently with

the production of a formal Surgeon General's report. But it is, indeed, similar in the sense that it involves input from multiple extremely knowledgeable individuals in various parts of the government. And then it's subjected to an external review as part of the process of being published.

Q Did Dr. Burney set out formal criteria for causation when he issued his statement in 1959?

A I believe that Dr. Burney in his 1959 statement used the standard approach to establishing scientific causality, which was to assemble all of the information and reach a judgment. He did not feel compelled, nor did he describe, the specific, detailed philosophical criteria or scientific criteria that he used, nor did he assemble the data within the context of each of those

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criteria in order to do a formal proof of causality.

The first time that I'm aware that that was done for smoking and health in that kind of exquisite detail was the 1964 report.

Q Are you aware -- strike that.

Was there, in fact, a consensus within the scientific community prior to 1964 about what the formal criteria were for reaching a judgment about causation?

A There were a variety of criteria that were available. The -- and scientists had established a basis for defining a causal relationship for a great deal of time prior to that, particularly initially with experimental science, and also with derivative or non-experimental science, such as mathematics, where things are derived from basic proof.

The question that arose with the introduction of epidemiologic studies was how these studies which were intrinsically not experimental studies could be used to establish causal inference on the relationships of various characteristics in the population to disease inference -- to disease occurrence.

There were a series of criteria that were developed by individuals. I believe Dr. Lillenfeld developed some. The ones that are most commonly cited

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are those of Bradford Hill. All of those criteria were then examined and synthesized into the ones that were used in the 1964 report. Those criteria were principally intended to address the issue of how you integrate epidemiologic data with the more traditional controlled experimental data in order to derive causal inference.

Q Prior to 1964 were there -- was there a scientific consensus on the formal criteria that should be used in reaching a judgment about causation using epidemiological evidence?

A There was an ongoing discussion with multiple sets of criteria that were in general widely accepted. Those criteria are the ones that I have described for you by Bradford Hill and by Lillenfeld. They were in evolution during that time, as was the acceptability of epidemiological data.

Okay. But there were widely accepted criteria for the use of epidemiologic data in drawing causal inference. That was something that was growing and

developing throughout that time and reached a crystallization, if you will, with the '64 report. But that is not to say that there was not a consensus prior to that as to initially whether or not epidemiological

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data could be used, and if so, what kinds of criteria would be used.

MR. SCHROEDER: Would you mark that one?

BY MR. BERNICK:

Q Are you aware of any consensus statement that was issued -- strike that.

Are you aware of any statement that was issued reflecting the consensus of the scientific community on the formal criteria that should be used for reaching a judgment about causation based upon epidemiological evidence?

A I don't understand your question. Perhaps you could clarify it for me.

Q You've described -- strike that.

I'll just put it to you again. I'm asking for whether there was a statement, a formal or any other kind of statement, an article that came out, a statement that was made, that reflected a consensus within the scientific community prior to 1964 on the formal criteria that should be used in reaching a judgment about causation based upon epidemiological evidence? Can you identify such a statement?

A Other than the ones that I have identified by Bradford Hill and Dr. Lillenfeld? I mean, those are

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formal statements. They were publications, okay? They --

Q Talk --

A They laid out criteria. You're asking me for some group statement?

Q Yes. I'm asking for a statement that reflects a consensus of the scientific community on what those formal criteria should be. Was there such a statement prior to the Surgeon General's report in '64?

A Well, you've asked multiple pieces there. There clearly was a scientific consensus that epidemiologic data could be used. There clearly was a scientific consensus that there were ways to use it.

The formalization of that process, the specifics as to how one would examine a body of epidemiologic data, was codified by Bradford Hill and by Lillenfeld and by others. Those -- I'm not aware of a specific formal group that accepted the responsibility for defining how that criteria would be used, okay, prior to the criteria that -- or the process used in '64 by the Surgeon General's report.

Q Maybe I'm not being clear. I understand your testimony that Bradford Hill wrote criteria. I understand your testimony that Lillenfeld published on

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the subject.

What I want to know, was there any statement of a consensus within the scientific community as to what the formal criteria for causation should be based upon epidemiological evidence?

A The first public group that was tasked with developing formal criteria for defining causality in the context of epidemiologic data for purposes of public health, okay, that I'm aware of, the first formal process that went through with a public organization that said, "This is something that we agree with," was the 1964 Surgeon General's report.
Q Are you familiar with work that was done by Dr. Bruslo (phonetic)?
A I am.
Q Who is Dr. Bruslo?
A Dr. Bruslo was one of the early investigators in tobacco and health who became director of health in the State of California and dean of the school of art -- of public health at UCLA.
Q Is he a person who was involved with this process of developing criteria for causation?
A I have no knowledge of his specific contribution in that area.
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Q Do you know what his involvement was in the field of smoking and health?
A He published one of the very early epidemiologic studies that showed that cigarette smoking increased the risk of disease.
Q And what's his area of specialty?
A His area of specialty is public health.
Q If Dr. Bruslo were to have said as recently as 1996 that a new set of criteria for thinking about disease causation were needed prior to the 1964 report, would that be a reasonable statement?
A That would depend on the context in which you are attempting to apply that statement.
Q Are you familiar with his retrospective review of the significance of the Surgeon General's report in 1964?
A I'm not specifically familiar with that, no.
Q The Royal College report in 1962 of the consensus statements that you've referred to prior to 1964, the Royal College's report in 1962 is really the only one that actually involves a group of experts getting together to focus on the causation issue and write a report of their findings; correct?
A It is the first report that I am aware of of a
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major organization convening an external group to specifically review the data and write a report on that data. It is not the only group that has involved a series of experts coming together to form a judgment, but it is the first one that went through that kind of formal process, sanctioned by an external -- by an organization and involving individuals external to that organization.
Q Now, the Royal College report actually came out before the Surgeon General's advisory committee was even formed; correct?
A That is correct.
Q And really actually what prompted the formation of the Surgeon General's advisory committee was the issuance of the Royal College report; correct?
A I don't believe that that was a major factor in

defining the need for the Surgeon General's report. My understanding from discussions with people who were involved at that time was that the concern had been that the science had become lost in the politics of the tobacco issue, and that it was important to define the scientific base clearly and definitively in order to move public health forward. And that was the purpose of the Surgeon General's report.

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Q Let me ask you this: At the time that the Royal College report had issued in 1962, isn't it true that the current statement of the Surgeon General in the Public Health Service on the issue of causation was still the statement issued by Dr. Burney in 1965 in the JAMA article?

A I'm not sure what you're asking. There had been -- there had not been another formal statement after that time.

Q Okay. That really was what I was trying to get at. Prior to the time the Surgeon General issued the 1964 report, the only statement of the Surgeon General on the issue of causation was the statement that appeared from Dr. Burney in the 1959 JAMA article; correct?

A I think that's an unfair characterization. The official statement was the one that was made in 1959. No official statement had been put out after that. I'm quite certain that there were other statements that were made in speeches and in other publications, but they were not formal positions, such as the one in '59.

Q So that the 1959 statement of Dr. Burney in the JAMA article was the position of the Surgeon General, the formal position of the Surgeon General on the

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causation issue until the '64 report was issued; fair?

A I think again that's a mischaracterization. The way science works is that you develop a position based on the data that exists in 1959 and you put that position out. It is then the position of the Surgeon General based on the data that is available to the Surgeon General in 1959. There was no review and revision of that between 1959 and the '64 Surgeon General's report, so there was not another statement. That is not to say that the statement in 1959 reflected all of the information that was available in 1963 or '4.

Q Well, isn't it a fact that the Surgeon General was specifically asked to issue a new statement on causation in 1962?

A The Surgeon General, I believe, was asked in 1962 to do that.

Q Okay.

A And because of the political context of tobacco in the U.S. political environment, felt that simply a statement by the Surgeon General as an agent of the administration would not be sufficient to satisfy the political conflict that was going on. In that setting, the Surgeon General felt that it was more appropriate to convene an external group that was independent and to

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have that external group examine the data and reach

conclusions and then submit that to the Surgeon General rather than it being the position of the Surgeon General, who is a branch, after all, of the administration.

Q Are you saying that Dr. Burney was not permitted to make a new statement in 1962 politically?

A I don't think that's a fair interpretation of the response I gave you.

Q Well, I wasn't seeking to interpret the response. I'm trying to understand the facts. Are you saying that Dr. Burney was not permitted politically to make a new statement about causation in 1962?

A That wasn't what I said.

Q Well, do you believe that that's true, that Dr. Burney was not permitted in 1962 politically to make a new statement about causation?

A I believe that Dr. Burney made a judgment that the political environment was such that a statement by the Surgeon General would not be one that would be the most effective approach; that he felt that the most effective approach would be to convene an external review. And that is what he chose to do.

Q Are you aware of any documentation of the

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deliberative process, that is, the thinking of the Surgeon General, about whether to issue a new statement in 1962?

A I am not specifically conversant with a document that describes that.

Q Now, if we go back and take a look at Dr. Burney's statement in 1959 in JAMA, are you familiar with that statement?

A I'm generally familiar with it. If you would like me to discuss specifics of it, I would like to have it in front of me.

Q Okay. I will do that, and I've actually already highlighted it to tell you in advance what particular parts of it I'm going to ask you about.

A Okay.

Q And I'd like to direct your attention, first of all, to a statement that was made by Dr. Burney. It appears in his conclusions. And he has a conclusion section, does he not?

A If you would like me to describe the document, it would be very helpful for me to have the document where I can see it.

Q I'm going to give it to you in just a minute. Do you recall he's got a conclusion session?

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A If you would like me to describe the document, it is a very simple process to look at it.

MR. GRUENLOH: And again, this is what I brought up before. This does impair the deposition, by you not having extra copies of the document.

MR. BERNICK: Thank you, Counsel.

MR. GRUENLOH: No. You had time last night certainly to get extra copies of the document.

BY MR. BERNICK:

Q The conclusion section states, and I will give it to you to read for your own, quote, "The Public Health Service believes that the following statements

are justified by studies to date: Number 1, the weight of evidence at present implicates smoking as the principal etiological factor in the increased incidence of lung cancer." Do you see that statement as part of Dr. Burney's conclusion?

A I believe that you have read that correctly.

Q Use of the word "implicates," was it reasonable for Dr. Burney to say that the weight of the evidence implicates smoking as a causative factor? Was that a reasonable word for Dr. Burney to use in 1959?

A I don't know what you're saying. It obviously is a reasonable English language word. In the context

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of this sentence, it fits. It is traditional in governmental language with lots of extra words contained in every statement. I mean, what is it that you're asking me to offer an opinion on?

MR. SCHROEDER: Mark that.

BY MR. BERNICK:

Q I just asked you whether that was a reasonable word for Dr. Burney to use in talking about the weight of the evidence on causation?

A In what context?

Q In the context in which you wrote it in 1959, as an official statement?

A I believe that the statement is an appropriate statement, including all of the words contained within the statement, when viewed in context of the entire sentence.

Q Are you aware of anyplace in the official statement that Dr. Burney issued in the JAMA article in 1959 where Dr. Burney says smoking causes disease?

A I believe that a reasonable English language interpretation of the phrase, "The principal etiologic factor in the increased incidence of lung cancer" is a statement that cigarette smoking causes disease.

Q That's your interpretation; correct?

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A That is my understanding of the English language and scientific language that is expressed by those words, yes.

Q It's true, is it not, that in 1964 the Surgeon General made the statement, "smoking causes disease," in just those words?

A Yes, he did.

Q Is there anywhere where you see in the statement made by Dr. Burney in 1959 that simple declaratory statement by Dr. Burney saying "smoking causes disease"? Is that anywhere in the document?

A It is not in the document. There are good reasons why it's not in the document.

Q Are you aware of anyplace prior to 1964 where the Surgeon General of the United States said in those simple terms "smoking causes disease"?

A I am unaware of the Surgeon General using the word "cause" in a public statement as an official position of the Public Health Service prior to 1964. It is my understanding and opinion that the statement contained in this volume, or in this paper, the "principal etiologic factor," is an appropriate scientific description that is intended to mean that

cigarette smoking produced directly a rise in lung
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cancer. That is a statement of causation.
MR. BERNICK: Move to strike as not responsive, the last sentence.
Q I'm asking a very simple question. I want to know whether the statement that appeared in the 1964 report that smoking caused disease is a statement that had been made by the Surgeon General in any context prior to 1964?
MR. GRUENLOH: Objection; asked and answered. He answered it. He's entitled to explain his answer.
Go ahead and answer it again, Dr. Burns.
THE WITNESS: Okay. It is my opinion that this statement in this page is a statement of causality. The word "cause," for reasons that are easily understood, was not used by the Surgeon General prior to the 1964 report.
BY MR. BERNICK:
Q Do you --
A At least in an official position statement.
Q Are you aware of anywhere where there is set out in writing the reasons why Dr. Burney chose to use the words that he did in his 1959 statement that we've been talking about, which is that the weight of the evidence implicates smoking as an etiologic factor?
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A I'm not specifically conversant with whether Dr. Burney left a journal or other descriptions. And I do not know of a specific document that described his thoughts when he chose those specific words.
Q Are you aware of anyplace prior to 1964 where the National Cancer Institute issued a statement publicly saying "cigarettes cause disease"?
A I'm not aware of the National Cancer Institute using the word "cause" in an official statement in the context of tobacco and disease prior to 1964.
Q Let me ask you, Dr. Burns, if you would flip through to see -- strike that.
The American Cancer Society prior to 1964 said "smoking causes disease"; correct?
A I believe that they said that following the publication of their studies in 19 -- early 1950s. Mid 1950s.
Q The American -- strike that.
The American Public Health Association also said prior to 1964 "smoking causes disease"; correct?
A I would not be surprised that that's true. I don't have a specific recollection of a statement at that point.
Q Do you know whether the Public Health Service
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generally and the Surgeon General in particular were specifically asked to make the statement "smoking causes disease" prior to 1964?
A Without some context for that -- I'm certain that somebody asked them. I don't know if -- they probably got letters. They probably got all kinds of things. I think you're asking for formal request by someone.

Q Yeah. By Congress.
A I'm not specifically conversant with a formal request of the Surgeon General. I would not be surprised that that happened. That was part of the ongoing political context at that time that led to the need for the 1964 Surgeon General's report.
I don't have a specific recollection of a specific document that describes a specific request using the word "cause."
Q Let me just ask you a question, Dr. Burns, the Public Health Service in general and the Surgeon General in particular are the -- is the highest health, public health office in the United States; correct?
A The Surgeon General is the chief officer of the U.S. Public Health Service. At the time that the Surgeon General's report was released he was a chief

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officer over the administrative side of the agencies of the Public Health Service as well as the uniform branch of the U.S. Public Health Service. He was not the chief health officer. That was the secretary of health and human health services, at that time the secretary of the Department of Health, Education and Welfare.
Q The obligation of the Surgeon General of the United States, the principal obligation of the Surgeon General of the United States all the way up through 1964 was what?
A The principal obligation of the Surgeon General of the United States was the running and administration and direction of the U.S. Public Health Service.
Q I see. You don't think that the principal obligation of the Surgeon General up to and as of 1964 was to protect the public health?
A That is the obligation of the Public Health Service, among other obligations. The principal obligation of that position is to run the organization that does that.
Q Okay. Because the Surgeon General is supposed to run the organization that does that, you don't think that the Surgeon General has as his principal obligation prior to 1964 to guard and watch over the public health?

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A I mean, I don't want to engage in some kind of semantic debate. Certainly --
Q Neither do I.
A Certainly the person running the organization is committed to, obligated by, and intent on fulfilling the missions of the organization. You asked me what the position did, and I told you.
Q So we're in agreement that recognizing that the Surgeon General was running the organization, that because the principal obligation of that organization was to guard and protect the public health, that was also the obligation of the Surgeon General prior to '64; correct?
A Certainly.
Q Now, I think we talked about yesterday the fact that by the early 1950s that smoking and health was a major public health concern; correct?
A It certainly was.
Q And as a major public health concern, it

20 required a major public response; correct?
21 A Yes, it did.
22 Q And certainly part of a responsible public
23 health activity and response was to tell the public in
24 candid terms the nature and extent of the hazard posed
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1 by cigarettes; correct?
2 A That is correct.
3 Q And part of telling the public about the nature
4 and extent of the hazard posed by cigarettes is being
5 candid about whether smoking causes disease; correct?
6 A I don't know what you mean by candid.
7 Either --
8 Q Being forthright about whether smoking causes
9 disease; correct?
10 A That is correct.
11 Q Now, the Surgeon General of the United States,
12 charged with the responsibility of protecting the public
13 health --
14 A Yes.
15 Q -- should certainly have said in simple terms,
16 "smoking causes disease," if he believed, in fact,
17 smoking causes disease; would you agree with that?
18 A I think that you have an incomplete and,
19 perhaps, somewhat naive understanding of the way the
20 process of government works.
21 The Surgeon General, as an individual, would
22 certainly be inclined to speak what he believed. The
23 Surgeon General, as the director of the Public Health
24 Service, is required, okay, in his official statements,
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1 to represent the Department of Health, Education and
2 Welfare and the political administration of the
3 president at that time. That constrains his or her
4 public statements on a variety of different issues. And
5 oftentimes they do not have free rein to decide what
6 would be the best method for achieving that mission,
7 because other individuals at higher levels of government
8 have other opinions about the priorities for the
9 executive branch of government, and have, being elected,
10 the authority to exercise those priorities.
11 That creates an obligation on the part of the
12 Surgeon General to be direct and forthright. It also
13 requires that obligation to be fulfilled within a
14 complex political construct that includes the secretary
15 of Department of Health, Education and Welfare, the
16 executive branch of government and the U.S. Congress.
17 Q Do you have any written evidence from any
18 source, Dr. Burns, that the Surgeon General's stated
19 views on the causation issue prior to 1964 were
20 constrained or limited in any fashion by political
21 considerations?
22 A As I sit here, from memory, without aid, I
23 don't have a specific recall of a specific document. My
24 recollection is that there has been a variety of
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1 documents written since that time and discussions with
2 the Surgeon General at that time, that indicate the
3 political pressure that was brought to bear with the
4 production of the '64 Surgeon General's report. But I

don't have them in memory. I don't have them here with me. And I don't have them by unaided recall as a specific defined document.

Q Have you -- strike that.

Do you have any evidence that Dr. Burney himself believed prior to 1964 that causation of disease by cigarettes had been proven?

A It is my understanding from reading the article that you have placed in front of me, that the English language conclusion as the official position of the Surgeon General, was one that states that the direct result of cigarette smoking was the increase in lung cancer. Okay? While it is possible that Dr. Burney signed off on that recommendation without his believing in it, I think that it is highly unlikely.

Q So the only evidence that you really have of Dr. Burney's views, his own personal views, are the one -- is what he actually wrote in the JAMA article in 1959; is that fair?

A You asked me for evidence. I provided you with

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this piece of evidence. I'm assuming that there is also evidence that exists. I don't have from unaided recall a specific document or a specific reference to give you that would define that.

MR. GRUENLOH: Do you need a break, Dr. Burns?

THE WITNESS: I think it would be helpful, sure.

MR. GRUENLOH: Before we break. In light of the court's ruling yesterday, I understand why you're plowing all of this old ground on causation and when the scientific community reached a consensus. But I really think it is old ground, and I'm going to lodge an objection as to scope here. If you want to take up all your time in this deposition on old material, that's fine. But it was my understanding that you were going to depose him, and I think you said this, on new material and material that's related to this case, specifically asbestos and synergy.

MR. BERNICK: If we would get a stipulation from counsel that the only issue in this case is asbestos and synergy, I won't ask the witness any more questions --

MR. GRUENLOH: That's not what I said. I think I refined what I said.

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MR. BERNICK: Excuse me, Counsel. I did not interrupt you. I'm not aware of any ruling that the court made at this point that bears upon this deposition. If there is some ruling that the court made as to what we can cover in this deposition, I'm all ears. But I'm not aware of anything like that.

These views, as I understand it, are directly germane to what's in his report. So I'm going to ask him the questions, but --

MR. GRUENLOH: Nevertheless, I will lodge an objection as to scope. I think this is old ground, but it's your time.

THE WITNESS: Let me make it clear that I'm unwilling to conduct an interminable deposition today.

MR. GRUENLOH: Before we do take a break, I

noticed that we were joined by another defense lawyer. If you could --

MR. MOLSTER: My name is Charles Molster from Winston & Strawn on behalf of Philip Morris.

MR. GRUENLOH: Thank you.

MR. MOLSTER: Can I just have you tell me what ruling you're referring to?

MR. GRUENLOH: It's my understanding that there was a ruling yesterday requiring the defendants, I think

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it was within 15 days, to respond to an interrogatory on causation. And I think that bears upon the issue that Mr. Bernick is asking.

MR. BERNICK: That's almost laughable. Could you explain to me how that ruling has any bearing on this deposition?

MR. GRUENLOH: Well, I also direct you to the opening statements of the Engle trial. I don't know if you've gotten a chance to read those yet, but I think they're directly contradictory to what you're trying to prove today. But the testimony will bear that out.

MR. BERNICK: Let's go off the record.

THE VIDEOGRAPHER: Off the record at 10:12 a.m.

(Recess.)

THE VIDEOGRAPHER: We are back on the record at 10:24 a.m.

BY MR. BERNICK:

Q Dr. Burns, at about the mid point of Dr. Burney's statement in 1959 on the causation issue he has a heading that says, "Criticism of the smoking hypothesis - not all investigators are in agreement with the conclusions reached by these researchers."

Do you see that?

A I see that that is what is written, yes.

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Q In fact, it's true, is it not, that there were limitations on the scope of the epidemiological evidence that had been gathered during the 1950s; correct?

A I'm not quite sure what your question is intended to imply. The --

Q Well, then I'll clarify.

A Okay, go ahead. That would be helpful.

Q In the early 1950s, these epidemiological studies came out linking smoking to disease. We've already talked about those; correct?

A That's correct.

Q And there were also mouse skin paintings that had been published; correct?

A That's correct.

Q All I'm asking is, the epidemiology that was available on the linkage of smoking and disease in the 1950s, that was epidemiology that had some important limitations to it; correct?

A All epidemiology and all science has limitations. I'm not quite sure what you're intending to state. Certainly the evidence that has come out since that time has been more complete, has expanded information, has generated larger populations that were examined with a variety of other constraints, so that

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there was information available after that period that was more complete, more detailed, more specific, over a broader range of populations, etc.

Q But would you agree that the epidemiological studies that had been done prior to 1964 had not been based on scientifically-designed samples and were, therefore, subject to the criticisms that the findings could not be generalized to the total population? I'm reading from the '79 report.

A I think that that statement is technically true. None of the epidemiologic studies, nor any epidemiologic study that I am familiar with, is truly a representative sample by the nature of follow-up required for an epidemiologic study or criteria for selection for an epidemiologic study. And therefore, they cannot be extrapolated to the U.S. population for purposes of estimating the frequency of disease in that population. That is not the purpose for which those studies were used in conjunction with defining the relationship of smoking and disease.

Q I didn't ask you that. I just -- really all I asked you was the question of whether the studies, the epidemiological studies that had come out prior to 1964, had important limitations, given the way that they were

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designed as described in the '79 Surgeon General's report?

A Well, I mean, I thought I answered both of those questions. If I didn't, give me someplace where I didn't clarify what I thought on those issues.

That sentence, you read correctly, I'm willing to state that.

Q Out of the '79 report?

A Out of the '79 report. You read it correctly. It is true that the epidemiologic studies -- and I'm aware of no epidemiologic study that has follow-up that is representative of the U.S. population. The purpose of drawing a representative sample is so that you can extrapolate the results, the percentage results to the U.S. population.

Q Were --

A That is a limitation for purposes of defining the prevalence of disease in the U.S. It is not necessarily a limitation for causal inference.

Q The views that Dr. Burney set forth in his 1959 statement on causation, those were views that other scientists who had published in the area didn't agree with; correct?

A Dr. Burney cites several scientists who

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disagreed, that's correct.

Q In point of fact, if we go back to the early 1950s, there are a series of scientists -- I'm going to go through a few of them. A series of scientists who issued articles or reports on the causation issue and expressed the view that causation had not yet been proven scientifically; correct?

A I believe -- maybe I misheard your statement. But I think you're asking me whether they expressed the opinion causation had not been proven?

Q That's right.

A Okay.
Q Is that correct, those people had -- there were people who issued articles and statements, scientists, to that effect during the 1950s?
A During the 1950s, and subsequently, there have been a continued stream of individual articles who adopted that position.
Q Okay. Now, if we go back to 1954, is it true that in 1954 articles were published by Dr. Huber of the National Cancer Institute on the question of smoking and health?
A I'm sure that Dr. Huber published articles in the 1950s.

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Q Okay.
A If you would like me to comment on a specific article, it would help me to be able to see the article that you're referring to.
Q Let me just ask you generally: Are you familiar with Dr. Huber's articles and publications during the 1950s on smoking and health, generally?
A I have a general understanding of Dr. Huber's articles. I have not reviewed those in sufficient detail to be able to cite them from memory, nor do I have an encyclopedic recall of all of the articles that he has published.
Q Dr. Huber was with the National Cancer Institute; correct?
A That's correct.
Q Do you remember what his position was within the National Cancer Institute?
A I'm sure he held several different positions. My recollection is that he was director of cancer etiology and prevention, something like that. I'd have to go back and look at his title.
Q Do you believe that Dr. Huber was qualified to address the issue of whether smoking causes disease?
MR. GRUENLOH: Object to the form.

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THE WITNESS: I've not reviewed Dr. Huber's specific qualifications in detail. I would assume that given his position, that the Public Health Service felt that he was qualified to offer an opinion.
BY MR. BERNICK:
Q Okay. Are you aware of any business or economic or other ties between Dr. Huber and the tobacco industry during the 1950s?
A Not specifically, no.
Q Another person who spoke to the issue of whether smoking has been proven as a cause of disease was Dr. Hammond; correct?
A Cuyler Hammond, yes.
Q And certainly Dr. Cuyler Hammond was qualified to address the issue of causation, was he not?
A Yes.
Q And Dr. Cuyler Hammond also issued fairly public statements setting forth his views on whether causation had been demonstrated during the early 1950s; correct?
A He has a number of statements throughout that period on the relationship, yes.

Q And you're familiar -- I know that you've been asked about this previously, but I want to set up a

different question. You've been asked about statements that he made in the course of an interview that he gave with U.S. News and World Report in 1954. Do you recall that?

A That's correct.

Q And some of the questions or some of the statements that I know that you've been asked about include his statement that, quote, "We are undertaking the project," that is, a follow-up research project, "because there is reason to suspect that smoking may cause lung cancer - we don't know it, but there is good reason to suspect it."

You're familiar with that statement that he made in 1954, are you not?

A Yes, I'm familiar with that statement. That statement was in conjunction with the epidemiologic research that he was about to publish at that point in time.

Q And we're now talking February, 1954; correct?

A I believe so. February 26th. That was probably -- the interview may have been some days prior to that.

Q Was it unreasonable for Dr. Hammond to make the statement that I just quoted out of the U.S. News and

World Report? Was it unreasonable for him to make that statement at that time?

A It was quite reasonable for him to make the statement that the scientific research he was about to publish had been undertaken as an epidemiologic research project in order to establish whether or not cigarette smoking caused lung cancer.

Q Dr. Wynder in February -- or April of 1954 also issued a public article, published in the Connecticut State Medical Journal. You're familiar with Dr. Winder, are you not?

A I am indeed.

Q And Dr. Wynder was a -- one of the people who authored one of those significant studies that came out in the early 1950s on smoking and health; correct?

A That's correct.

Q And he was with the Sloan Kettering Institute for Cancer Research?

A Yes, he was.

Q And he was also qualified to address the question of whether smoking caused disease; correct?

A Yes.

Q In April of 1954, he made the statement, quote, "The reasons for the apparent greater correlation of

cigarette smoking with lung cancer are not entirely clear."

Are you familiar with the statement that Dr. Wynder made at that time?

A Okay.

Q Do you have my question in mind?

A Perhaps you could rephrase it or reiterate it

for me.
Q Yeah.
A That it would be helpful.
Q The question that I asked you -- if you would give me the article back for just a second -- was whether it was reasonable for Dr. Wynder to state in his article in April, 1954, that, quote, "The reasons for the apparent greater correlation of cigarette smoking with lung cancer are not entirely clear." Was it reasonable for Dr. Wynder to make that statement in 1954, in April?
A It was reasonable for Dr. Wynder to make that statement in conjunction with his discussion of the differences in lung cancer incidence between pipe and cigar smokers.
Q You see that there's a further statement later on about mouse skin painting?

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A I do.
Q And that basically Dr. Wynder in 1954 in that article talks about some of the limitations on the use of data from mouse skin painting. Do you see that?
A I see that.
Q Was it reasonable for him to make those statements about the limitations on the use of mouse skin painting data in April of 1954?
A It was useful for him to make those statements.
Q I meant to say reasonable for him to make those statements in 1954.
A It was reasonable to describe the -- his data and the limitations of the interpretation of his data. Both of those are reasonable things for him to have done.
Q As they appear on the article that you're looking at, the April, '54 article?
A As they appear in the article where Dr. Wynder concludes that cigarette smoking increases the risk of lung cancer and he argues pervasively for prevention as a major reason -- major approach to this problem.
Q He's not the only person who pointed out the limitations of mouse skin painting, is he?
A He is not the only person who has done that.

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Q In fact, the American Medical Association issued an editorial in the same year that talked about the limitations of mouse skin painting; correct?
A I am not certain that that is true. I would not be surprised if that was a fact.
Q The American Medical Association was qualified to issue statements regarding smoking and health in the 1950s, was it not?
A It certainly had the opportunity to issue those statements. And it had the resources to credibly review the information.
Q Okay.
MR. SCHROEDER: Will you mark that one for me?
THE WITNESS: I mean, I can finish that answer if it would make you happy. The issue is whether or not the statements were actually based on a review or were based on the role that the AMA was playing at that time, which was a more political role in Washington in an

effort to alter the reimbursement for medical care.
And so it depends on -- they certainly had the potential. I've told you I didn't know the article you were -- in question. I didn't know the statement in question. You asked me whether they were in a position to do that. I told you that they had the potential to
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do it. Whether they actually did it would be dependent on the context in which they did it, a scientific review, in which they certainly would be entitled to do it. Or a political statement. In which case it might be driven by the political imperative.
MR. BERNICK: Could you read back the question to the doctor, please?
(Record read.)
THE WITNESS: As I just described. I stand by that answer.
BY MR. BERNICK:
Q I would just like to know whether they qualified or not. Not whether they were politically motivated, not whether they had the opportunity to. I'd just like to know whether the American Medical Association was qualified to address the causation issue in the 1950s?
A I think I answered that question fully. The American Medical Association had the resources and access to the expertise to review the information and develop a credible position. It would be an expert position on this issue.
Q Now, the American Medical Association has archives. They publish what's called the Archives of
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Industrial Hygiene and Occupational Medicine, or did in the 1950s; correct?
A I don't have an encyclopedic recall of all of their publications. I wouldn't be surprised if that were true.
Q You what?
A I would not be surprised if that were true.
Q Now, you earlier described the process of review or expert review that takes place in connection with editorials; that is, before an editorial is issued, it has to undergo review by other experts. Do you recall that?
A That's correct.
Q And you would certainly expect that the American Medical Association would follow that same process of expert review prior to issuing editorials; correct?
A In general that is true. The American Medical Association also produces as editorial statements of the American Medical Association. Those would be internally developed and are usually not subject to external review. But with that exception, they in general would undergo review.
Q Are you familiar with the editorial that the
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American Medical Association issued in June of 1954?
A Not by unaided recall without some prompting, no.

Q I'm going to show you the editorial that was issued in June of 1984 by the American Medical Association?
A Are you sure you meant 1984?
Q I'm sorry, 1954. Thank you. And ask you whether you're familiar with that?
A I am generally familiar with it. I can't tell you that I have read it in detail.
Q That was the official position of the American Medical Association at the time? Or was this simply an editorial by the two individuals who appear?
A I'm not certain who Robert Eckardt and Philip Drinker are, as to whether they are the officials of the American Medical Association or scientists who work in this area. It would appear as though it was an article that is developed by people asked to write the editorial, but I can't be certain of that.
Q There's nothing that purports to have that editorial be an official statement of the American Medical Association; correct?
A No, it here does not.

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Q So this is simply an editorial that appeared in the archives of the Industrial Hygiene and Occupational Medicine --
A Uh-huh.
Q -- correct?
A That's correct.
Q Directing your attention to the part of the first paragraph that I bracketed, and I'll just read it into the record for a moment if you'll give that back.
It says, "Various industrial dusts and fumes, tobacco smoke and exhaust gases from heavy automotive traffic have been suggested as possible factors in lung cancer. These materials have been tested chiefly on the skin of mice. However, the mere demonstration of materials in the air which are known to be carcinogenic to mouse skin is not proof they cause cancer -- that they can cause human lung cancer."
Was that a reasonable statement for this editorial to make in June of 1954?
A That is a reasonable statement then. It is a reasonable statement now. The isolated demonstration of a single exposure in mice is not sufficient evidence to establish human carcinogenicity.
Q Was it a reasonable statement for the editorial

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to make in 1954 that the factors that are listed, including tobacco smoke, were possible factors in lung cancer? Is that a reasonable statement to make in June of 1954?
A The statement contained in this editorial is an incomplete statement of the science that was available at that time.
Q I didn't ask you that. I asked you whether it was a reasonable statement for the editor -- for the editorial to make in June of 1954 that --
A My --
Q Excuse me, that cigarette smoking was a possible factor in the causation of lung cancer?
MR. GRUENLOH: I think again you interrupted

Dr. Burns in the middle of his answer. So if you could continue with your answer, Dr. Burns.

THE WITNESS: My understanding of the English language use of the term "reasonable" includes whether the statement was based on the evidence that was available at that time. Okay? If I'm not responding to your definition of reasonable, then please let me know, and I'll use some other definition. But is that your understanding of the term "reasonable" as well?

BY MR. BERNICK:

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Q I'm just asking for your view on whether it was reasonable for the editorial to state, as we have seen, in June of 1954, that smoking was a possible factor in lung cancer?

If you think it was reasonable, you say "yes." If you don't think it was reasonable, you say "no."

MR. GRUENLOH: And Dr. Burns told you he had a problem with the way that you were phrasing the question. I guess I need to object to the form of every one of these questions if we're going to do this. But he's telling you where he has the problem, what term you're using that he as difficulty with.

MR. BERNICK: That's a speaking objection. It's inconsistent with the rules of practice in this court.

MR. GRUENLOH: Object to form.

MR. BERNICK: Thank you.

Q Answer the question, please.

A My understanding of the term "reasonable" includes whether or not the statement is based on the facts that were available at that point in time. For the statement to be made that these materials have been tested chiefly on the skin of mice is a statement that is exclusively focused on the animal model testing of

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carcinogenicity. It's quite widely accepted then and now that animal model data in isolation of other data are not sufficient to establish human carcinogenicity.

The statement, however, ignores the fact that at that time there was substantial data available that established epidemiologically a relationship in humans. To ignore that data in making these statements, I think, defines the statement as incomplete and not based on the data that was available at that time, and therefore, not reasonably reflective of all of the evidence that could be brought to bear to answer this question.

Q Are you familiar with Dr. Robbins' textbook on pathology?

A I'm generally familiar with that.

Q What is Dr. Robbins' textbook on pathology?

A It is a textbook on pathology.

Q Is it regarded as an authoritative source within the field of pathology?

A It is a textbook. Textbooks are authoritative only in the sense that the data contained within them are authoritative. The texts themselves are not authoritative.

Q Dr. Stanley Robbins, is he a person qualified to speak to the issue in the 1950s as to whether smoking

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caused disease?
A I don't know the specific qualifications of Dr. Robbins to address that issue.
Q Have you looked into that?
A I have not.
Q Do you know whether Dr. Robbins, Dr. Stanley Robbins, who authored the textbook of Pathology, had any ties to tobacco?
A I'm not familiar with whether Dr. Robbins did or did not have ties to the tobacco industry.
Q Dr. Milton Rosenblatt, are you familiar with his publication on cancer of the lung in the 1950s?
A Not by unaided recall, no.
Q Do you remember anything about Dr. Rosenblatt?
A I would need more prompting than that for an isolated name. I don't generally store information by individual names.
Q Associate of professor of medicine, New York Medical College; visiting physician and chief of Chest Clinic, New York City Hospital; fellow of the American College of Chest Physicians; American College of Cardiology; New York Academy of Medicine; American Medical Association.
A So he was --
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Q Author of Cancer of the Lung, Pathology, Diagnosis and Treatment, published by Oxford University Press, 1956. Ring any bells?
A Not specifically, no.
Q The Burney statement itself in 1959 in JAMA, there was a response to that, was there not?
A I'm assuming that there was, yes.
Q Well, in point in fact, the editors of the Journal of the American Medical Association made a response to the Burney statement almost immediately after it was issued; correct?
A I don't have a specific recall of that.
Q Well, what is the practice when an editor responds to an article that appears in the journal? How does that editorial get put together?
A In that setting, it is usually an editorial written by the editor. Sometimes the editor will submit that for external review. Oftentimes they do not.
Q The editors of the Journal of the American Medical Association in 1959, when Burney issued his statement, were they qualified to address the issue of whether smoking caused disease?
A I don't know. They certainly had expertise available to them that they could draw on. Whether they
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as individuals were qualified to do that or not would be a matter of individual qualification. And I'm not familiar with their individual qualifications.
Q Are you aware of any ties between the tobacco industry and the editors of the Journal of the American Medical Association in 1959?
A Not as individuals. Certainly the American Medical Association had substantive ties to tobacco throughout that period and subsequently.
Q On December 12 of 1959 -- the date of the

Burney article itself was what? I think you have it in front of you there.

A I believe it is -- let me check the next page. November 28th, perhaps, 1959.

Q Okay. I want to put before you the Journal of the American Medical Association, December 12th, 1959, and ask you whether this contains the response of the editor of the Journal of the American Medical Association to Dr. Burney's statement earlier in the year?

A I believe that that's a response to the information, yes.

Q Basically the editor of the journal was issuing a response to what Burney had said in the statement that

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he made in 1959 in JAMA; correct?

A He was simply describing the context that he felt that statement fit.

Q Okay. And if you'd hand that back to me for a moment.

The editor says, in part, "That a number of authorities who have examined the same evidence cited by Dr. Burney do not agree with his conclusions." Was that a reasonable statement for the editor of JAMA to make in 1959?

A That was a reasonable statement for the editor to make. It was the same statement that Dr. Burney made in his own article.

Q He goes on to say, "Although studies reveal a relationship between smoking and cancer that seems more than coincidental, they don't explain why even when smoking patterns are the same, case rates are higher among men than among women, and among urban rather than among rural populations."

Was that a reasonable statement for the editor of JAMA to make in December of 1959?

A That was a statement that was reasonable at that point in time. And it is a statement that is intended to predicate the following statements, which is

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that you can't make an all-or-none statement, which is that all of lung cancer is caused by cigarette smoking. And no one has made that statement at any point in time that I'm aware.

Q And then it goes on to say, quote, "Until definitive studies are forthcoming, the physician can fulfill his responsibilities by watching the situation closely, keeping current of the facts, and advising his patients on the basis of his appraisal of those facts."

Would you agree that that's a reasonable statement for the JAMA editor to make in the December of '59?

A I think that that is a general admonition that would apply to all aspects of science and medicine then and now.

Q I'm just asking whether it was a reasonable statement for the editor to make in 1959?

A If it was reasonable then and now, is that not an answer to your question?

Q Can you just answer the question, Dr. Burns? Was that a reasonable statement for the editor to make

in JAMA in December of 1959?
A I think it a reasonable admonition for the editor of a journal to make to its readership of
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scientists and physicians at that time and currently.
Q Now, it was not only the editor of JAMA who responded to Dr. Burney's 1959 statement? There were others who also responded; correct?
A I mean, I don't know how to answer your question. Obviously the tobacco industry responded. There were other individual scientists who responded. There were probably other organizations who responded.
Q I'll be more specific. One of the other people who responded, one of the other scientists who responded to Dr. Burney's 1959 statement was Dr. Berkson (phonetic); correct?
A Dr. Berkson responded on repetitive occasions to the -- anyone's statement that cigarette smoking caused disease.
Q In fact, he wrote his own letter to the editor, which appeared in the subsequent addition of the Journal of the American Medical Association; correct?
A It would not surprise me in the least that Dr. Berkson did that at that point in time as he has done it at multiple occasions since that time as well.
Q Dr. Berkson was with the Mayo Clinic; true?
A I don't know specifically where he was located at that time.
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Q Have you looked into what his qualifications were to address the issue of whether smoking caused disease in the 1950s?
A I cannot cite without unaided recall Dr. Berkson's qualifications. My understanding is that he was a statistician scientist who was interested in the topic.
Q Well, he was also a medical doctor, was he not?
A My understanding was that his criticisms were largely in the area of statistics. He may have well been an M.D. As I said, I don't have a specific knowledge of his CV.
Q Do you know whether he was qualified to address the question of whether smoking caused disease in 1959 and 1960?
A I have not conducted a specific review of Dr. Berkson's qualifications for purposes of defining whether or not he was qualified. I have read several of his articles over time describing his concerns about the establishment of a causal relationship between smoking and health.
Q Are you aware of any ties that Dr. Berkson had to the tobacco industry?
A I'm not specifically conversant with his
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financial arrangements and whether or not he had any ties to the tobacco company.
Q Are you familiar with Dr. Louis Robbins of the cancer control program of the Public Health Service who wrote on the issue of smoking and health in 1962?
A I'm generally familiar with Dr. Robbins.

Q Dr. Robbins was as, I indicated, chief of the cancer control program at the Public Health Service in the 1960s; correct?
A I'm willing to accept your attestation that that was true.
Q Well, certainly Dr. Louis Robbins was a person who was qualified to address the issue of smoking and disease in the early 1960s, was he not?
A I have not conducted a detailed review of Dr. Robbins' background and CV. Certainly one would expect from his position at the public health, that the U.S. Public Health Service felt that he was qualified.
Q You're not aware of any ties between Dr. Robbins and the tobacco industry, are you?
A No, I'm not.
Q And it was the view of Dr. Robbins as of 1962 that a distinction had to be made in looking at the causation issue between the perspective of the medical

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investigator and the perspective of the medical practitioner; do you recall that?
A I recall that article. That was the reason -- it's my understanding that that was the reason why Dr. Burney chose to express the relationship between smoking and disease in the terms that he used rather than to enter into the fray about whether the term "cause" should be restricted for experimental proof as opposed to the general understanding use of that term where one event results in another.
Q It was the perspective of Dr. Robbins that unless you were able to demonstrate experimentally that cigarettes caused disease from the perspective of a medical investigator, you couldn't say cigarettes caused disease; correct?
A I think that's a mischaracterization of what he said. What he was saying was that for purposes of dealing with the issue of taking care of people, of preventing the disease, that the evidence was sufficient to be able -- the evidence that existed was sufficient to be able to say that smoking caused disease and that people should not smoke, and that that would prevent disease.
For purposes of narrowed philosophical use of

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the term "causation" as it is used by experimental scientists, that term, if one chooses to restrict it to pure experimental truth, would require experimental proof of causation prior to the use of that term.
I think he was drawing that distinction because it was a common philosophical debate at that time --
Q In --
A -- particularly in relation to use of epidemiologic data as to whether or not causation as a term should be reserved for a purely experimental proof or a derivative proof, as in mathematics, or whether causation could be used in the general English use of that term, which was that one exposure or one event directly results in another.
Q When you say "at that time," you're talking about 1962?
A I'm talking about what he is expressing in his

article at that time, yes.
Q And he states in the article, quote -- this is Dr. Robbins stating in his 1962 article, quote, "For the medical investigator, however, the evidence still does not add up to conclusive proof that cigarette smoking causes lung cancer." Right?
A What he says, very clearly, in his article, is
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that there are two positions. Okay? One is for purposes of public health, for purposes of dealing with individuals, for purposes of preventing disease. And the other is as a medical investigator for purposes of experimental proof, in a purely experimental scientific context.
What he says is that within the public health context, for purposes of dealing with people, that the conclusion is legitimate that cigarette smoking causes disease, and that that has been established. What he also says is that the pure experimental proof which requires an exploration of the mechanisms of disease occurrence and development of all of those chain of events has not been completed.
Q In that context, Dr. Burns, was it reasonable for Dr. Robbins to state in 1962, quote, "For the medical investigator, however, the evidence still does not add up to conclusive proof that cigarettes smoke causes lung cancer"?
A I believe I've defined what he said in the article in the appropriate context. I'd be happy to do that again. But I believe that that was -- is an appropriate discussion in the context as I defined it.
Q Dr. Burns, you yourself have drawn a
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distinction between proof of causation -- strike that. Between answering the question of whether cigarettes cause disease for purposes of scientific proof and answering the question of whether cigarettes cause disease for purposes of taking action; right? Am I correct that you yourself have drawn that distinction?
A I'm not quite sure what you're referring to. I certainly have, indeed, at various points in time expressed my opinion on causation.
Q Well, you testified specifically in the fall of 1998 that there was a difference between scientific proof which would meet the epidemiological standards, and enough of a showing of relationship between smoking and disease to take action; correct?
A I believe that that is correct, that there are multiple stages. There is a stage at which the evidence is sufficient to be concerned. There is a stage at which the evidence is sufficient to take action. And then there is a stage at which scientific proof has been established.
Q At what point in time do you believe that there was sufficient scientific proof of causation for a warning to be issued to the general public that cigarettes may cause disease?
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A Okay. It is my understanding that the issues of warning are legal issues. And I would not want to

offer an opinion about the legal basis, okay, for requiring a warning.

I believe that by 1950, or the early 1950s, individuals who were knowledgeable about the science that existed at that time were entitled to a reasonable, direct, and appropriate concern that it was likely that cigarette smoking caused lung cancer.

I think that by the mid 1950s, that reasonable people would be expected to draw a conclusion that cigarette smoking probably caused lung cancer, and that actions should be taken. And then certainly by the time of the '64 report, and more likely by the time of Dr. Burney's statement, the mid to late 1950s, the conclusion that this was a scientific fact, okay, was indeed one that I believe scientifically was the appropriate conclusion.

All of those should translate into warnings, as is appropriate under the law and precedent that exists. And I'm not knowledgeable in that enough to offer an opinion as to what specifically would be required. But medically and scientifically I have defined what I think the science would support at those points in time.

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Q And just so we're clear, you aren't aware of any statement by Dr. Burney prior to December of 1959; correct?

A I mean, we have covered this in -- on multiple occasions. That is the first official statement. I'm certain that Dr. Burney spoke and, perhaps, wrote on this in a less formal and less official context. That is the first official statement by Dr. Burney for the Public Health Service.

Q Could you identify for us any prior statement where he sets out a position of the Public Health Service on the issue, prior to 1959?

A I have told you that this is the first statement, official statement of the Public Health Service by Dr. Burney on this issue.

Q Now --

A That I am aware of anyway.

Q Now, we've had a lot of discussion about when there was a consensus of the scientific community on whether smoking caused disease; true?

A I think there's a fair statement that that has absorbed a bulk of our time, that's correct.

Q And I take it you would agree with me that the fact that there is a scientific consensus does not mean

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that there are no people who have different views on the subject?

A That is correct.

Q And certainly when it comes to the issue of whether causation had been proven scientifically, there were a number of scientists throughout the 1950s, and indeed, in the early 1960s, who felt that it had not been scientifically proven that cigarettes caused disease; correct?

A There were individuals at that time who held that position. There are individuals now who hold that position. They are indeed a dwindling few. But there are individuals even currently who hold that position.

Q Well, I want to focus on the period of time prior to 1964.
A Okay.
Q Would I be correct in saying that there were qualified scientists who had no ties to the tobacco industry who felt and published the view prior to 1964 that the causation of disease by smoking had not been scientifically proven?
MR. GRUENLOH: Objection to form.
THE WITNESS: As we have already demonstrated here with an individual that I agree had qualifications,
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who wrote an article in that time period, okay, that individuals expressed that opinion. Most individuals who expressed that opinion differentiated between the use of causality in a pure experimental scientific context, as opposed to the general English language use of the term, and therefore, were drawing that distinction, saying that the laboratory evidence was not sufficient to build fully the mechanistic causal link that established the relationship between exposure to the ultimate occurrence of disease.
That statement, okay, was true, then; it is true now. There are individuals who hold that position then; there are individuals who hold that position now. Okay? There were also individuals who felt --
Q Dr. Burney, this is -- you are going on and this is not responsive to my questioning. I asked you a simple question.
A I'm very flattered, but I am not Dr. Burney. Dr. Burney was a distinguished Surgeon General. I'm just a professor of medicine.
Q I think you get my drift. Should I put the question to you again?
A I think you are interrupting my question --
Q I was interrupting your answer because you were
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going on. It wasn't responsive.
MR. SCHROEDER: Would you mark that?
THE WITNESS: I was trying to be responsive to your question.
MR. GRUENLOH: Please do mark that. That was a perfect example of you talking over the witness. If you have a problem with his answer, define your question better. Object to form.
BY MR. BERNICK:
Q I'll put the question to you. Isn't it a fact that prior to 1964 there were qualified scientists with no ties to the tobacco industry who published the view that it had not been scientifically proven that cigarettes caused disease?
A In view of the extensive discussion we have just had of one of the papers of a scientist who did that, I think that the answer to the question is that, of course, there were scientists who did that. Most of the scientists, as I said, distinguished between experimental proof, which was what your question, as I understood it, to be directed at, from the English language use of that term "causality," whether or not the events or exposure directly resulted in the occurrence of the illness.

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Q How many such scientists were there?
A I don't have a count.
Q Do you know of anybody -- strike that.
Do you know of anybody who wrote during this period of time that those scientists who expressed this view, that is, that causation had not been demonstrated, were not acting in good faith?
A I know of no one who wrote that. I would not mean to imply that. I think that they were using a different set of standards for the philosophical use of the term "cause" within the scientific context, and they wanted to reserve that term for experimental proof.
Q Do you believe that their perspective was an unreasonable scientific perspective?
A I think that --
Q At the time?
A I think that it is a philosophical perspective about the use of the term. I don't believe that it would be reasonable for individuals as scientists to hold that we need a complete and total description of the entire biologic basis for the occurrence of disease before we have the knowledge to conclude that the disease is produced by a specific agent; and therefore, I think that if that is the standard and that is the
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standard that some were trying to apply, that is unreasonable.
If one is adopting a standard that there needs to be some understanding of the basic mechanism by which an exposure could result in the disease in question, I think that's quite a reasonable standard.
Q And that's certainly the perspective that was being articulated by Dr. Robbins in 1962; correct?
A I've outlined several perspectives. Which one are you referring to?
Q The last one, that there had to be some reasonable demonstration experimentally.
A No. Dr. Robbins said that the evidence for the general understanding, English language use of the term "cause," okay, for purposes of public health, for purposes of Dr. Burney's statement for public health, that the evidence was sufficient to draw a direct relationship.
He was saying, however, that the mechanistic understanding with experimental demonstration in the laboratory had not been completed.
Q I'll be more precise. The perspective that is described by Dr. Robbins in 1962 as the perspective of the medical investigator, was that or was that not a
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reasonable perspective in 1962?
A Reasonable for what purpose?
Q Reasonable for scientific purposes; that is, to be able to say for scientific purposes that causation has been demonstrated?
A It is certainly true that if one applies circular logic to that statement, which as I understand it is what your question is doing, that it is certainly true. That it is a reasonable thing for a scientist to

do to hold a standard and to expect that that standard would be met.

If you are asking me whether it is a reasonable for a scientist to hold that that experimental process needs to be complete, and that experimental use of the term "causality" needs to be complete before action is taken, before a statement of causality for purposes of public health is taken, okay, then I don't believe that that's reasonable, and I also don't believe that that is the position that was articulated by Dr. Robbins.

Q Okay. Do you believe that the medical investigator, the perspective of the medical investigator described by Dr. Robbins in 1962 was a reasonable perspective as he described it at that time?

A I am frankly at a loss as to what I have not

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communicated on this issue to you. I think that Dr. Robbins' position --

Q I didn't ask you that Dr. Burns. I'll try to be clearer.

A Okay. I don't understand your question. I'm sorry.

Q Dr. Robbins describes the perspective of the medical investigator; correct?

A He describes the perspective of the medical investigator with a specific use of that philosophical approach to mean the restriction of the proof of scientific causality to experimental proof in the laboratory.

Q All I'm asking is, was the perspective of the medical investigator, as described by Robbins in 1962, was that a reasonable perspective?

A It is reasonable perspective for a scientist working in an experimental context to continue to work in that context to develop a full understanding of the causal chain of events between something that is known to cause a disease and the subsequent occurrence of that cause of disease; that indeed, is a very clear and very reasonable action on the part of an investigator.

It is also reasonable, as Dr. Robbins

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articulates, to say that for purposes of public health, we can define this causal relationship while we are in process of examining experimentally the precise mechanism by which that process has occurred. And he acknowledges that many people would reserve the term "cause" until that experimental demonstration has occurred. All of that is something that I would find to be reasonable.

Q Dr. Burns, you've talked to -- strike that.

You've made mention, I believe, in your -- I'll tell you what -- strike that.

Let me try to get one more thing, and then we'll change tapes.

Are you familiar with Dr. Clarence Cook-Little?

A I am -- I know who he is. I've never met him.

Q Did Dr. Clarence Cook-Little act as the scientific director for the organization called the Tobacco Industry Research Committee?

A I believe that he did. I'm not -- I thought it was council, but it may have been committee.

Q And he issued a series of statements in the 1950s on behalf of that organization regarding smoking and health, did he not?
A He did.
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Q And in order to set out his views clearly, he actually published an article in Atlantic Magazine in 1957; correct?
A I'm not specifically conversant with that article.
Q It was the view of Dr. Little throughout the 1950s that the causation of disease by smoking had not been scientifically demonstrated; correct?
A I believe that that was Dr. Little's opinion throughout his entire tour of association with the Tobacco Research Council.
Q Now, isn't it also true that when Dr. Little expressed that position he did so expressing the position and views of the scientific advisory board of that same organization?
A I am not sure that that was true.
Q Did you take a look to see whether the articles when published specifically cited that the views were approved by the SAB?
A I did not specifically review that. I'm aware that subsequently that process was not followed. I don't know specifically what statement was approved or how it was referred to. If you could help me with that, I could offer more reasoned opinion.
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Q Yes, I'll show you the -- see if you can refresh your recollection. I'll show you Exhibit -- the article out of the Atlantic in December of 1957, which you'll see is by Dr. Little on the specific issue of whether he's stating simply his own position or also the position of the Scientific Advisory Board. You may want to take a look at page 76 and the paragraph immediately over the heading, quote, "The right to learn and to inform."
A I'm not sure where I'm supposed to be looking here.
Q Just above the --
A This page? Page 76? 75?
Q Yes.
A I was reading page 75. I'm sorry.
He notes in his article, I guess in the Atlantic Monthly, "It is important for the public to remember that members of the Scientific Advisory Board in their approach to this research responsibility take the position that smoking has not been proven guilty or guiltless in matters affecting human health." Since --
Q Do you want to finish what he says is their view?
A I'd be happy to. "Their attitude is that
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statistical and indirect evidence does not prove its guilt as a causative agent. The open question of its innocence or its guilt can best be answered through unhampered scientific research for the full facts."
Q Are you aware --

A The science advisory board at this time was set up in response to the Frank statement, where the Frank statement by the tobacco industry said, "The evidence has not established that smoking causes any disease." It would not be surprising that individuals who signed on to the Scientific Advisory Board would be willing to accept that position by the tobacco companies.

What he says in his article, however, is that the Science Advisory Board for the purposes of examining the basic science has adopted the position that there is neither proof nor lack of proof. That is the basis of any scientific investigation. You don't enter into a scientific investigation if you know the results already. That is not a statement that the Science Advisory Board endorsed Dr. Little's positions then or in perpetuity on whether or not it was reasonable for purposes of public health to decide that there was a causal relationship between smoking and disease.

MR. SCHROEDER: She's got to change the tape.

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THE VIDEOGRAPHER: We're going off the record at 11:24 am.

(Recess.)

THE VIDEOGRAPHER: This marks the beginning of Videotape Number 2 of Volume 2 in the deposition of Dr. David Burns. We are back on the record at 11:29 a.m.

BY MR. BERNICK:

Q Dr. Burns, are you aware of any evidence that anybody from the SAB ever stated or suggested prior to 1964 that Dr. Little had misrepresented their views?

A I don't believe that that statement was made, no.

Q Okay. If we focus on this period of time that is prior to 1964, are you aware of any evidence that any of the views expressed by the tobacco industry on causation of disease had been acted on by any doctor for any medical association?

A That question is so broad that it cannot possibly be answered.

Q Okay. Well, let me break it down.

A Okay.

Q Are you aware of any doctor or medical association prior to 1964 who ever recited the views of

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the tobacco industry on the causation issue and said that he or they agreed with those views?

A From unaided recall, I can't produce from memory a specific document that includes all of those pieces in it. It certainly was commonly true at that time that individuals such as Dr. Berkson reiterated the same arguments that were being made by the tobacco companies and placed them in the context of a denial of the establishment of scientific causality.

I am uncertain as to whether any of those articles actually discussed the tobacco industry's position as part of the basis of forming the body of evidence. But the evidence being cited by those individuals was essentially the same as the positions adopted by the tobacco industry.

I am also aware, but don't have from unaided

recall, that many of the professional organizations adopted positions consistent with those of the tobacco companies, and that those positions were influenced by tobacco industry positions even though that influence was not acknowledged directly in the position statements.

Q Are you aware of any actual evidence that the positions taken by any doctor or medical association on

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causation was, in fact, influenced by statements made by the industry on that question?

A While I don't have a specific document in front of me or available by unaided recall, it is my understanding that there was a substantial interaction between the U.S. tobacco companies and the American Medical Association during and subsequent to the 1964 period, that is construed by many, including myself, to have influenced their positions.

Q I'm talking about before 1964. Are you aware of any actual evidence that the views of any doctor or any medical association were influenced by statements made by the tobacco industry concerning the causation issue?

A My understanding is that that relationship with the AMA antedated the 1964 period. I don't have a specific document that I can recall from memory that establishes that to be true.

Q Are you aware, is there any such document that appears in your report or in any of your reliance materials?

A I did not address that specific issue in either my report or my reliance materials.

Q Are you aware of any actual evidence that any

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doctor, medical association, or public health authority acted, that is, took action, on the basis of the industry's views about causation prior to 1964?

A It is my understanding from discussions with people who were involved at that time that the position taken by Dr. -- taken for the development of the 1964 Surgeon General's report was a substantive response to the articulated position of the tobacco companies that smoking causation had not been established, and that that was an action that was taken directly in response to the tobacco industry's statements and their political influence to implement those statements in the U.S. Congress, and that that was the purpose for conducting such an extensive and elaborate process to examine this issue in 1964.

Q Do you have any actual documentary evidence of that?

A As we sit here with unaided recall, I don't have a specific document. I have discussed that with people who were participants at that time, but I don't have a specific document that I can point to as we sit here with unaided recall.

Q Well, is it anywhere in your report or your reliance materials?

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A I did not address that specific issue in the

reliance materials or in the report.
Q Apart from what you say is the action undertaken to basically produce the '64 Surgeon General's report, are you aware of any actual evidence that any doctor, medical association or public health authority took action on the basis of the industry's views about causation prior to 1964?
A It is my understanding, and I don't have the document in front of me, and can't recall it with unaided -- from unaided memory, that the positions of the American College of Chest Physicians, among others, were influenced by the positions of the tobacco companies, as expressed by representatives from those states that -- where tobacco was a major crop in the formation of those opinions. I don't have a specific document that describes that deliberation, nor a specific document that defines that influence.
Q Are you aware of any public health authority who took any action based upon the industry's expressed views about causation prior to '64 other than what you say is the action that was taken to produce the 1964 Surgeon General's report?
A What action are you asking me to --
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Q Any kind of action at all. Are you aware of any actual evidence that any public health authority took action prior to 1964 based upon the industry's expressed views about causation?
A You're asking for an action that they took based on the absence of an effect? I mean --
Q No, I'll rephrase the question again. I don't -- I want to avoid a long dialog about this. I'm asking you for something that I think is relatively simple.
The industry expressed its views, that is, the tobacco industry expressed its views about causation prior to 1964; correct?
A Yes.
Q All I'm asking is, are you aware of any action that public health authorities either took or failed to take prior to 1964 as a result of their relying upon the views expressed by the tobacco industry on the causation issue?
A Okay. It is my understanding that the U.S. Public Health Service and various other agencies took some actions to deal with the tobacco issue prior to 1964. The extent and effectiveness and magnitude of those actions, it is my opinion, was substantially
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influenced by the fact that there was an ongoing political debate directed and orchestrated by the tobacco industry as to whether or not the science had established causality. That interfered with delivery of public messages and effective public action on the part of various agencies.
I don't have a specific document in mind that establishes that that influence interfered directly with a specific action.
Q The 1964 report clearly was a landmark report; correct?
A It was an important document.

Q Well, the Surgeon General in 1989 called the '64 report a landmark event; correct?
A He did.
Q And you'd agree with that statement; correct?
A I have no reason to disagree with that statement in the context in which he used it.
Q The 1989 Surgeon General's report also talks about the impact of the 1964 report, does it not?
A I'm certain it does, yes.
Q In fact, in 1989, the Surgeon General describes the mobilization of resources to deal with smoking and health and basically get people to quit smoking; right?

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A Yes. Following the production of that report, and its conclusion, there was a substantially greater effort on the part of the voluntary health agencies and others to deal with the public health problem defined by the Surgeon General's report.
Q In fact, in the 1989 report, -- which was the 20th Surgeon General's report; correct?
A I'm perfectly willing to accept that number.
Q It describes the fact that there has been "dramatic progress that has been achieved in the past quarter century against one of our deadliest risks." Would you agree with that statement as it was made in '89?
A I would agree with that statement as it was made in '89 in the context in which it was made.
Q Would you agree that the -- throughout the period of time, that is, since 1964, tremendous changes have occurred? Would you agree with that statement made by the Surgeon General in 1989?
A I would agree with that statement globally, and I would agree with that statement as the Surgeon General made it, I believe, in the context of what has happened relative to smoking behavior.
Q Would you also agree that the changes that are

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described by the Surgeon General in 1989 as changes that have taken place from 1965 to 1989 in smoking behavior represented nothing less than a revolution in behavior?
A I think that that is a fair statement. There have been substantial changes in smoking behavior over that period of time.
Q Would you also agree with the statement made by the Surgeon General in 1989 that the anti-smoking campaign has been a major public health success?
A I would agree with that.
Q Would you also agree with the achievements that are cited by Surgeon General in 1989 in the field of smoking and health had few parallels in the history of public health?
A I think it is true in multiple context that they had a few parallels. They had a few parallels in terms of the magnitude of the disease created. They had few parallels in terms of the ability to change and mobilize society in changing smoking behavior. And they had a few parallels in terms of a focused and organized opposition on the part of a specific industry.
Q The achievements that are described in the '89 report are achievements that nowhere make reference to

24 opposition by the tobacco industry; correct?
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 1 A I don't believe that that's true. I believe
 2 that the opposition of the tobacco industry is defined
 3 in the body of the report.
 4 Q Is there any aspect of tobacco industry conduct
 5 which is addressed in the '89 report other than
 6 advertisement?
 7 A I believe that advertising and lobbying efforts
 8 are addressed in the report.
 9 Q Anything else?
10 A I would have to go back to the report to
11 examine other issues.
12 Q Well, certainly one of the things that was done
13 after the '64 report was that there were more Surgeon
14 General reports; correct?
15 A I don't know what you are saying in relation to
16 a causal inference. Certainly subsequent Surgeon
17 General's reports occurred after 1964.
18 Q A total of, what is it, 23 reports have now
19 been issued?
20 A I don't keep a running number in my head. It's
21 probably somewhere in that number.
22 Q And the basic goal of those reports was to
23 review all the available scientific evidence on smoking
24 and health; correct?
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 1 A That's correct.
 2 Q To make summaries of those, of that data --
 3 summaries of the data that were accurate and complete
 4 and balanced; correct?
 5 A That's correct.
 6 Q And to basically reflect and recite the current
 7 state of the art in smoking and health science; correct?
 8 A That's correct.
 9 Q Is it true that the volume of research that is
10 analyzed and digested in the different Surgeon General's
11 reports is vast?
12 A I think that's a fair statement.
13 Q Literally thousands of different pieces of
14 scientific research are cited, analyzed, summarized and
15 encapsulated in recommendations by the Surgeon General
16 since 1964; correct?
17 A That is correct.
18 Q The scientific community looks at the Surgeon
19 General reports as being authoritative; correct?
20 A In general they do, yes.
21 Q Is there any more reliable authority in the
22 field of smoking health since 1964 than the Surgeon
23 General reports?
24 A I think that they are a quite credible, quite
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 1 reliable, and are generally regarded as such. I know of
 2 no other set of reviews of smoking and health
 3 information that is considered more detailed and
 4 authoritative.
 5 Q Are you aware of any public health authority
 6 since 1964 who relied upon views stated by the tobacco
 7 industry when they were in conflict with pronouncements
 8 or statements by the Surgeon General?

A Perhaps you could tell me what you mean by "relied on."
Q Took action or failed to take action based upon something said by the tobacco industry.
A There were multiple examples that I am familiar with, okay, surrounding the issue of regulation with environmental tobacco smoke, where positions were adopted by committees of public health and safety in various jurisdictions, where the opposition by the tobacco companies and the statement by the tobacco companies that there was not scientific proof that established that environmental tobacco smoke caused disease was used as a basis for not imposing protective regulations to prevent exposure to environmental tobacco smoke. That is one example that I am familiar with in some detail where the reliance by individuals who are

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responsible for public health on tobacco industry synthesis of information and expression of that synthesis in terms of whether or not there was a causal relationship influenced public policy, influenced the actions on the part of people.

There are other instances, including testimony in front of Congress for a variety of different pieces of legislation. I cannot cite them from memory with unaided recall, but they certainly exist. There are other instances where choices were made by agencies at the state and local level about allocation of resources into various preventive efforts. It is my opinion that those allocations were influenced by both the political power of the tobacco companies and their failure to acknowledge the existing scientific statement that smoking caused disease.

Q Today, isn't it true that the Congressional Research Service does not believe that it has been established that environmental tobacco smoke causes disease?

A I don't believe that that's a fair reading of those reports.

Q Well, what do you think the view of the Congressional Research Service is?

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A I think that the review of the Congressional Research Service in its reports was that they don't believe epidemiologic data in one report. And in another report they established that various studies had various limitations and they did not draw a conclusion.

I'm not aware that they drew a conclusion that there was evidence after a review of all the evidence to conclude that there was not a scientific relationship established between smoking and -- environmental tobacco smoke and disease.

Q Well, let me put my question, then, in the following way: Congressional Research Services specifically looked at the issue of whether environmental tobacco smoke causes disease, does it not?

A I'm not sure that I would accept that characterization. They accepted a charge from Congress to examine certain specific aspects of the data that exists. And they produced two reports on that specific charge.

Q Are you aware of any public health authority since 1964 who has taken as true any statement made by any tobacco company on smoking and health issues where it is -- where the statement by the tobacco companies is in conflict with Surgeon General reports?

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A I mean, that's a very strange statement for me to respond to. I think that it is true, during the period of time that I've been involved in public health, that very few public health authorities have been willing to accept any statement by a tobacco industry representative that was in conflict with the Surgeon General's report as something other than an attempt to misrepresent the scientific data.

That is not the same thing as saying that those individuals were not constrained in terms of their actions by the political position adopted by the tobacco companies that the science had not proven causality.

But I don't think that in their heart of hearts as a scientist they believed that the tobacco industry was even under the remotest of circumstances telling the truth when it said that the scientific case had not been established.

I think it is, perhaps, one of the great corporate tragedies that for an industry as large as the tobacco industry, that the representatives of the public health community nearly universally expected to be lied to when they were spoken to by the public -- by the tobacco industry.

MR. SCHROEDER: Would you mark that one for me, please?

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BY MR. BERNICK:

Q Do you know of any medical association or public health authority since 1964 who has regarded the tobacco industry as a reliable source of information on the question of whether cigarettes cause disease?

A I'm not sure what your question is intended to reflect. There are certainly a number of individuals who have relied on the tobacco industry scientists for certain components of the information.

Q Dr. Burns, I'm sorry. I listened now to two different answers that were long and nonresponsive. And you've now told me again that you're not sure what it is that I'm asking. I'll withdraw the question.

A Okay.

Q And I'll put it to you again very, very simply. Because I'd just like an answer to it.

Are you aware of any medical association or public health authority which since 1964 has regarded the tobacco industry as a credible source of information on the question of whether cigarettes cause disease?

A I am uncertain about the period immediately following 1964. But certainly for most of the period that I have been involved in this issue, from 1975

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onward, my experience with individuals in public health is that they universally expect that the tobacco company will lie to them about the relationship, scientific relationship and scientific evidence establishing that

cigarette smoking causes disease.
Q Are you aware of any medical association or public health authority since 1964 who has looked to the tobacco industry as a credible source of information on the question of whether cigarettes cause disease?
MR. GRUENLOH: Objection; asked and answered.
THE WITNESS: Could you define for me what you mean by "information"? When I've tried to answer that question in terms of content information, you've told me that that's not what you want. What is it that you're asking me to respond to?
BY MR. BERNICK:
Q Information supplied by the tobacco industry, views expressed by the tobacco industry on the question of whether cigarettes cause disease.
A Okay. Are you restricting that to the statement, "cigarette smoking does not cause disease"? Or are you including within that the body of information on the component parts of the science used to establish that? Those are two -- many people, the Surgeon

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General's report included, have relied on published data from the tobacco companies about the composition of cigarettes, about smoke chemistry, about a variety of other factors, that are important pieces of the information that establishes that smoking caused disease. That is, in general, relied on and felt to be within certain bounds credible. That's not the same thing as relying on the tobacco industry's statement that the science has not established that smoking causes disease.
Q Okay, that's fair enough. Now, I'll rephrase the question. Are you aware of any doctor, medical association or public health authority who since 1964 has regarded the tobacco industry as a credible source of statements regarding whether causation of disease has been established?
A Okay. I am aware that when these issues come up before public health authorities for purposes of altering any form of public policy or developing any major tobacco intervention, that it is common for tobacco industry representatives to testify in front of those hearings. They are, in general, treated respectfully in that setting. And it is my opinion that their testimony forms the basis for actions, or more

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appropriately inactions, on the part of many of those bodies.
I am not aware of a voluntary health agency, with the exception of the AMA, who from 1964 on has said that, "We believe that the tobacco industry statement that smoking does not cause disease is a credible statement."
If I'm not answering your question, tell me where I'm missing the point, and I'll do my best to answer it. But that's as complete as I can be.
MR. SCHROEDER: Mark that.
BY MR. BERNICK:
Q Are you aware of any doctor, medical association, or public health authority since 1964 who has stated publicly that causation of disease from

smoking has not been established?
A My understanding is that that statement was made by the American Medical Association in conjunction with a gift by the tobacco industry for its research and education fund; that they were maintaining the position that the causation remained to be established.
Q Do you have any documentation of the American Medical Association since 1964 taking the position that causation has not been established?
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A I don't have a specific document from recall or sitting here in front of me that I can cite.
Q Are you aware of any situation where any doctor, medical association, or public health authority since 1964 has recited the views of the tobacco industry on causation as being true?
A It is my understanding that a variety of health committees for unions, a variety of Congressional committees, a variety of other organizational structures, have produced descriptions of the relationships of smoking and health, in particular environmental tobacco smoke and health, an area that I'm more conversant with specifically, that were close to identical to those of the tobacco industry. And that therefore, they were either parroting those or accepting them and using them as a basis for their opinion. I am unaware of a specific voluntary health organization which has done that.
Q Are you aware of -- setting aside environmental tobacco smoke, just talking about mainstream smoke, that is, actually smoking, are you aware of any doctor or medical association or public health authority that has taken the statements made by the tobacco industry about the health effects of smoking as being true since 1964?
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A I mean, I'm having some difficulty figuring out where I haven't answered your question. Help me.
Q You asked me before -- it's a very broad question, is what you're saying. I'm now just breaking out very specific things to ask you so that we don't get into dialog about what it is I'm asking you and the scope of it. I'm just asking you very, very simple pieces.
This piece, and I'll say it again, are you aware of any doctor, medical association, or public health authority that has taken statements made by the tobacco industry on the question of causation since 1964, has taken those statements as being true? And I want to set aside for purposes of this question environmental tobacco smoke.
A It is my understanding that those authorities responsible for regulating advertising, for increasing taxation, for a variety of other public policy interventions which are felt to be effective in altering smoking behavior, have accepted as valid arguments by the tobacco companies that advertising does not influence adolescent behavior --
Q Dr. Burns, I didn't ask you about advertising and adolescent behavior. I'm asking you about causation
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of disease.
A Okay.
MR. BERNICK: Let's just mark the record. I'm not even going to pursue this line.
THE WITNESS: That's fine.
MR. BERNICK: We'll just take it up with the judge. I'm just tired of going through this.
THE WITNESS: I had thought I answered your question completely on causation of disease.
MR. BERNICK: No. I asked you about causation of disease, and now you're talking about advertising. They have absolutely nothing to do with one other. I can't continue on this basis. I'll just asking you something else and we'll take it up with judge.
THE WITNESS: Well, if you had allowed me to complete my answer, I might have been able to show you how they had something to --
MR. GRUENLOH: You know, Dr. Burns, we've got it on the record. Let's go ahead.
THE WITNESS: Okay.
BY MR. BERNICK:
Q Are you aware of any doctor, medical association, or public health authority that has recited the industry statements regarding addiction as being

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true since 1964?
A I don't understand your question. There are several organizations that have not said that smoking was addictive during that period of time. That was the position of the tobacco industry at that period of time. I don't believe that those organizations relied exclusively on the tobacco industry's analysis of that body of information to form that opinion.
Q I didn't ask you whether they relied exclusively.
A Look, I'm trying to figure out what it is you're asking.
Q You're a very smart man; you know exactly what I'm asking.
A I don't.
Q The tobacco industry makes a statement about addiction. They've done so since 1964. True?
A That's correct.
Q All I'm asking you is, are you aware of any doctor, any medical association, or any public health authority that have recited those positions, those tobacco industry positions as being true on that subject?
A I don't understand your question. Can I

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clarify where I don't understand it?
Q Sure.
MR. GRUENLOH: Would you like help on this?
THE WITNESS: Okay. I don't understand what you're asking me. There are organizations which have clearly expressed the same position as the tobacco industry. Okay? That is true. Therefore, in that answer there are many organizations who have said that. Okay? If you're asking me whether they took the statement by the tobacco company and said, "We believe the statement by the tobacco company as a tobacco

company statement is true," I don't think any organization would ever do that with any statement by any external group. They would examine the data internally.

And so certainly organizations have made statements on addiction that are -- "we do not believe that cigarette smoke -- that nicotine is -- we do not know whether the addictive agent in cigarette smoke is nicotine," or that "we don't know whether cigarettes are addictive." That is the same position maintained by the tobacco companies. But I don't believe that they took that position from the tobacco company statement.

BY MR. BERNICK:

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Q Are you aware of any doctor, medical association, or public health authority who formulated their position on addiction since 1964 on the basis of statements made by the tobacco industry?

A I am unaware of any physician or organization that would formulate their opinion in that way, and therefore, I am not aware of any organization that has done that.

Q Are you aware of any doctor, medical association or public health authority that has formulated its position on whether cigarettes cause disease since 1964 on the basis of any statement made by the tobacco industry on that question?

A "Any statement" is restricted to causation?

Q Causation of disease.

A Okay. Agencies and groups of professionals who examine this issue for purposes of reaching a judgment, examine the data upon which that judgment is reached. They do not rely on external statements by, particularly, an industry group as the basis for drawing their conclusion.

So since no organizations form opinions in that way, I know of no organization that did form its opinion in response to the tobacco industry statement. Or used

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or relied on the tobacco industry's statement in synthesis of the data on causality as a principal piece of the information that they used to formulate their opinion.

Q It's been in the Surgeon General reports in 1960 -- since 1964 that tobacco smoke contains various carcinogens; correct?

A That's correct.

Q The fact that tobacco smoke contains carcinogens has been no secret in the scientific community since the very early 1950s; correct?

A That is correct -- well, mid 1950s, that's correct.

Q Well, in fact, even before the mid 1950s, the fact that tobacco smoke, for example, contained benzpyrene was published in Reader's Digest in 1950; correct?

A It is correct there was an ongoing scientific discussion at that time as to whether or not it came from the tobacco or from the paper and its exact derivation. But certainly -- I mean, there's no substantive reason to quibble about this. By the mid

1950s it was clear that it came from the tobacco. It was evident in tobacco smoke from cigarettes that -- as

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they were conventionally manufactured much earlier than that.

Q It's been no secret from the scientific community that cigarettes contain additives since the same period of time, long before 1964; correct?

A I think that that's an overly broad statement. It was generally understood by people who were interested that things were added to tobacco. Up until fairly recently what was added to tobacco, and specifically what was added to tobacco for individual cigarettes, was considered a trade secret and not available to the general scientific community.

Q Well, a whole series of additives are described in the 1964 report; correct?

A That's correct.

Q So the -- while the full list of additives may not have been set forth in the '64 report, the 1964 report does disclose the fact that a variety of additives are used in the manufacture of cigarettes; correct?

A It does describe that some additives are used. It also does not make any attempt to portray its description of additives as either comprehensive, complete or fully discussed in terms of its

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implications.

Q Is it true that Reynolds published a long list of additives used in the manufacture of cigarettes in the early 1970s? Have you looked at that?

A I have looked at a list. There is a very long list of things that might be used. As I said, it is my understanding from fairly detailed knowledge that the tobacco companies did not disclose what constituents, what additives were actually used, the amount that were actually used, and particularly the amounts that were actually used for individual brands of cigarettes until quite recently that. That those issues were defined by the tobacco companies as trade secrets and not to be made publicly available to the general scientific industry.

Q Are you a familiar with the fact that in 1984 the tobacco industry submitted to the Department of Health, Education and Welfare a list of additives used in the manufacture of cigarettes?

A I'm quite familiar with the fact that they submitted that list. And that requirements for submission was that the list be kept in a safe, not disclosed, and that no more than five individuals total could ever see the list.

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Q I didn't ask you that. Did they submit a list --

A You asked me whether I was familiar with it.

Q Do you want to have the question -- let's have the question reread.

A Okay.

(Record read.)

MR. SCHROEDER: Would you mark that, too?
BY MR. BERNICK:
Q It's a yes or no question, Dr. Burns.
A I don't believe that's a yes or no question.
Q "Are you or are you not familiar" is a yes or no question.
A I think that you believe it is. I believe -- when you ask questions in a context, they occasionally need an explanation.
Q Are you aware of any additives actually used in the manufacture of cigarettes that were not included in the list that was provided to HEW in 1984?
A I have never seen the list provided to HEW in 1964 and was not --
Q 1984?
A 1984. And therefore, am not in a position to answer your question. That list, as I said, was
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restricted to five individuals. I was not one of those individuals.
Q Dr. Burns, let's talk a little bit about the formation of the TIRC. You're familiar that that ultimately became known as the CTR; correct?
A That is my understanding.
Q And CTR was an organization that in part provided grant funding for people to do scientific research; correct?
A In part, that's my understanding.
Q And the formation of the CTR or TIRC grant program was announced through what's known as the Frank statement that was published in early 1954; true?
A Well, as I understand it, technically they announced the intent to form it, but yes.
Q Are you familiar with the fact that Hill and Knowlton was consulted in connection with the formation of the TIRC or CTR?
A I am.
Q Have you looked at the documentation that was associated with the formation of the TIRC and the issuance of the Frank statement?
A I have seen some documents that relate to that. I'm not specifically certain what documents you're
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referring to.
Q Have you made an effort to gather all the documentation that's available in the litigation pertaining to the formation of TIRC and the issuance of the Frank statement?
A I made an effort to collect as much of that information as I could find. It is my understanding from review of that that it is an incomplete description of all of the documents that are likely to be available. Or likely to have been available at one point in time.
Q Have you heard the claim that the formation of the TIRC/CTR was a public relations effort?
A Yes. And since Hill and Knowlton was a public relations company, that is consistent with contracting with them to help form it.
Q Do you know whether the formation of the TIRC was something that was Hill and Knowlton's idea?
A I'm not sure what you're -- what you're saying.

Hill and Knowlton was contacted by, as I understand it, the tobacco companies to help them deal with this issue. I don't know specifically whether an individual at Hill and Knowlton recommended the formation of this scientific group, whether that came from individuals within the tobacco companies, or whether that was part

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of a joint discussion that identified that as a reasonable course of action following their meeting.

Q Okay. I guess what I'm asking is, do you have any opinion or will you be offering any opinion that the formation of the TIRC was Hill and Knowlton's idea?

A If asked, I would give the same answer that I just gave you. I don't expect to be asked that question.

Q But if asked, would you say that it was, or would you say that you just don't know?

A I have answered the question for you. Okay? I would give you the same answer to the same question --

Q Well, I'm just not sure. I don't understand -- I guess I'm not clear on what your answer really is.

A My answer was that I don't know whether or not the formation of the TIRC, okay, was generated by someone from Hill and Knowlton as a recommendation, whether it was generated as a recommendation by one of the individuals from one of the tobacco companies, or whether it was a product of the joint discussion that occurred at the meeting.

Q One of the purposes that is stated in the Frank statement for the TIRC -- the purpose that is stated in the Frank statement for the TIRC is to sponsor the

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conduct of scientific research; correct?

A I believe that's correct.

Q Do you have any evidence, Dr. Burns, that when the TIRC was formed and the Frank statement was issued, that the intent of the tobacco industry was only to achieve public relations purposes and not to conduct scientific research that was bona fide and relevant to the issue of smoking and health?

A That is a complex and multiple-part question that I'll be happy to address. The -- it is my understanding that a principal reason for the TIRC being created was a public relations reason. And that the potential conflict between its public relations activity and the efforts to present it as an objective scientific resource were what led to the creation of the lobbying group, the Tobacco -- I'm blocking on it now. That's awful. They were around so long. Tobacco Institute, to separate those two activities.

It is my understanding that they attempted to fund credible research, that I don't know the specifics of the intent of the initial funding as to whether that initial funding was specifically directed at answering the questions about smoking and disease.

The grants that were funded and the research

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produced from those very initial studies would suggest that there was an effort in that direction. Subsequent research funding would lead to a different conclusion.

But your question related to the initial organization of it, and that's what I'm trying to respond to it.

Q Let's break this down, because maybe I didn't put the question as accurately as I should have. I'm talking about the purpose that the tobacco industry had in forming the TIRC. That's what my focus is. What was intended at the time TIRC was formed. We'll just work with that to begin with.

A My understanding --

Q I haven't put a question.

A I'm sorry.

Q That's just my focus.

A Okay.

Q At the time that the TIRC was formed, do you have any evidence that the tobacco industry did not intend to conduct bona fide scientific research through the TIRC?

A It is my understanding that there was an intent to conduct legitimate scientific investigation through the TIRC in the grants funding program. There was also an intent to use the TIRC as a public relations activity

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to support the tobacco industry's position on the causation of disease. That both of those were part of the reasons for forming the TIRC. But certainly, I believe that there was an effort made to fund scientific research that was credible, and in the early stages was related to tobacco.

Q The TIRC was set up to have a Scientific Advisory Board; right?

A That's my understanding, yes.

Q The basic organizational structure was then that the tobacco industry would provide money to the TIRC, that the TIRC would receive grant proposals from outside scientists, that the SAB would review and approve them, and then funding would be provided to the approved grant recipients. Did I get that right, as you understand it?

A That is not my understanding of how the process worked. That is my understanding of what was presented to the public as to how the process worked. My understanding of how the actual process worked is that there was considerable oversight by the tobacco companies, and particularly the tobacco company lawyers, as to what activities the TIRC funded.

Q Let's break that one down again too.

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At the time the TIRC was created and throughout the existence of the TIRC and the CTR, there has been a Scientific Advisory Board; correct?

A That's my understanding.

Q And the function that was to be served by the Scientific Advisory Board was to approve grant applications, correct?

A That was the purported function, that's correct.

Q And isn't it true that the idea of having a grant program, with grants approved by the scientific -- by a group of outside experts, that that was something that was actually recommended to the tobacco companies by outside scientists in the early 1950s?

A It would not surprise me that was true. I don't have a specific document that establishes that. But it would certainly be the kind of advice that outside scientists would give to the industry.

Q What the tobacco industry committed to in the Frank statement was that the Scientific Advisory Board would be comprised of distinguished men from medicine, science and education who would be invited to serve on the board; correct?

A I believe that is what they committed to. I

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don't believe that they meant to restrict it to men.

Q Okay. Now, in fact, if we take a look at the people who have served on the Scientific Advisory Board over time -- have you examined the people who served on the Scientific Advisory Board over time?

A I've not done a comprehensive evaluation of each of the boards during each of the years. I'm generally familiar with the individuals on those boards, and many of them have distinguished scientific reputations.

Q Are you aware of any member of SAB, that is, anybody who served on the SAB, who wasn't qualified to make grant funding decisions in the field of smoking and health?

A I guess that depends on what you mean by the "field of smoking and health." There certainly were scientists who were on that board who were well qualified to make funding decisions on basic science that could ultimately have some relationship to tobacco. As to whether or not they were qualified to specifically target research to answering the question of whether smoking caused disease, I think that the boards, as selected, were not selected to contain a body of individuals who would be focused on answering that

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question. They were largely selected from individuals who were principally focused on the basic mechanisms of disease occurrence, and therefore, were interested in basic science rather than answering the applied question of whether tobacco caused disease.

Q Well, regardless of what the direction of the research was, were there any members of the SAB, that is, people who actually served on the board, who are not distinguished men from medicine and science and education?

A I've not conducted a detailed review of every individual who is on the board. It is not my understanding that the board was composed of individuals who do not -- who did not meet that criteria. I have no individual in mind who did not meet that criteria.

Q Are you aware of any evidence that any member of the SAB felt that they were part of a scientific process that was not really a bona fide scientific process, but was being pursued in bad faith?

A I have not seen documents from or correspondence from the SAB expressing their individual opinions on that issue, either pro or con.

Q Are you aware of anybody from the SAB who has ever spoken out and said that they understood their job

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on the SAB was to support an effort to deceive the public?
A I have never heard such a statement, and it would be very surprising for an individual to -- who is sitting on a board to make that kind of statement.
MR. SCHROEDER: Would you mark that?
BY MR. BERNICK:
Q Are you aware of who the scientific directors of the CTR have been?
A I can't from memory give you a complete list. I know that there are -- have been several.
Q Do you recall that the first scientific director of the CTR was Dr. Clarence Cook-Little?
A I recall that.
Q Do you recall that he was on the -- he was the director from 1954 to 1971?
A I don't have those specific dates in memory. I'm perfectly willing to accept that those are true.
Q Is it true that Dr. Little was former president of the University of Michigan, former president of the University of Maine, founder of the Jackson Memorial Laboratory, managing director of the American Cancer -- The American Society for the Control of Cancer, which became the American Cancer Society, and president of the
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American Association for Cancer Research?
A I've not conducted a detailed review of Dr. Little's background. The description you have given me is, indeed, consistent with my understanding of his background.
Q Would it be fair to say that Dr. Little, the first scientific director, was a man of unimpeachable integrity and national repute?
A I think that in the light of events that have occurred over time, there has been some question about whether that would be a fair characterization of Dr. Little. I think at the point of time at which he was appointed there would not have been a substantive criticism on that basis.
Q I see. You don't believe that Dr. Little acted with integrity?
A I think that it is difficult for me to believe that an objective scientist of -- acting with integrity would have continued to adopt the positions that Dr. Little was adopting. And it is my belief, okay, without a document to substantiate it, that his responsibilities and interactions with the tobacco industry colored his objectivity in terms of deciding what the science had concluded.
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Q I want to ask you very specifically. Integrity pertains to somebody's personal ethics; correct?
A I'm referring to scientific integrity, which is whether or not the conclusions that you are expressing are based upon the data that you have reviewed, or upon your personal perspectives and biases about what those conclusions might be interpreted to mean. I'm not suggesting that Dr. Little was guilty of any kind of financial irregularity or that he was accepting payments in order to alter his positions.

Q Do you believe that Dr. Little was acting in bad faith in the activities that he pursued as the first scientific director of CTR?
A I think Dr. Little was acting in very good faith relative to the expectations of the people who had hired him. I think that he was not acting in good faith as a scientist, interpreting data for the public about whether cigarette smoking caused disease.
Q Do you believe that Dr. Little knew that he was part of some campaign to deceive the American public?
A I think that what Dr. Little knew or didn't know is beyond my ability to define with any degree of precision. I think that having looked at the evidence myself, and having reviewed some of Dr. Little's

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statements, it is very difficult for me to believe that his objectivity in reviewing that data was maintained and that his review of that information was not influenced by his association with the tobacco industry, and that, therefore, he was not fulfilling his obligation as a scientist to examine the data objectively, and he was not fulfilling his responsibility to the public to communicate what the scientific community believed to be true on the issue of smoking causing disease.
Q Do you believe that Dr. Little intentionally deceived the American public on any issue?
A I don't have a specific knowledge of Dr. Little's state of mind at the time he made those statements. What I've said is that the statements are not consistent with what I believe to be an objective review of the scientific data available.
Q I'm just asking, as an expert, are you prepared to say that Dr. Little attempted to deceive the American public?
MR. GRUENLOH: I think Dr. Burns has said that he has no idea what his state of mind --
MR. BERNICK: This is another speaking objection, Counsel.

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MR. GRUENLOH: Object to form.
BY MR. BERNICK:
Q Do you want me to put the question to you again?
A Certainly.
Q Do you believe that Dr. Little was acting to deceive the American public?
MR. GRUENLOH: Object to form.
THE WITNESS: I believe that Dr. Little's actions deceived the American public as to the state of the scientific knowledge on whether smoking caused disease. I am not in a position to define Dr. Little's state of mind in order to characterize whether that was something that he had convinced himself to be true, or whether he was saying that, having known that it was not true. So I cannot tell you what his intent was.
I can tell you that those actions did not reflect the scientific consensus at that time, and that my view of that scientific information would lead me to believe that it is very difficult that an objective scientist with the responsibility of reaching that

judgment would have reached the conclusions Dr. Little did and express them. And therefore, I raised the question as to whether he was acting in a way that was

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objective and independent of his association with the tobacco industry.

BY MR. BERNICK:

Q Are you familiar with any of the other scientific directors, Dr. Gardner, Dr. Summers, or Dr. Glenn?

A I have never met them personally. I am generally familiar with them.

Q Did any of them lack the qualifications to act as scientific director for CTR?

A I am assuming that since the -- since CTR hired them based on a set of criteria, that they fulfilled those criteria. I believe that each of them had a credible scientific background and were credible in the position to administer a grant program.

Q Are you aware of any deceptive statements that were made by Dr. Gardner, Dr. Summers, or Dr. Glenn?

A I don't have a specific knowledge as I sit here of a specific statement at a specific point in time. It is my understanding that they persisted in the statement that the science had not established that smoking caused disease. But I don't have a specific instance in mind.

Q It's true, is it not, that the grant program ultimately executed by -- or I should say funded by CTR

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was an extensive one?

A It certainly funded a substantial body of research and provided a substantial level of funding.

Q I want to set aside the question of research conducted into the central nervous effects of smoking. I want to set that to one side.

A Okay.

Q Are you aware of any evidence that the SAB at any point in time was told what they could or could not approve by way of grant proposals or contract proposals?

A My understanding was that the SAB did not approve all of the aspects of the funding of monies that came through TIRC. Of those that came through the SAB for approval, they voted on those, and then there were administrative decisions about what got funded.

There was also some selection as to what grants were consistent with the mission, and therefore, what grants would be submitted to the SAB. I'm not aware of an instance where the SAB was told that they could not fund studies in certain areas; that that was done through other mechanisms besides telling a group of scientists that they cannot do something. And that my review of the documents on that issue leads me to believe that the people running the TIRC recognized that

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they could not tell a distinguished group of scientists what they could not fund. And that there were other mechanisms that could be used to change the nature of the work that was funded.

Q Setting aside research regarding the central nervous system effects of smoking, are you aware of any

grants that were approved or contracts that were approved for funding by the SAB, but they ultimately were not funded by the CTR?
A It is my understanding that there were grants routinely approved for funding that were not funded. That is standard with most grant processes.
Q Name one. You say that that's standard. I'm not asking you about what is standard. I'm asking you about CTR.
A I don't have a specific grant in mind on a specific SAB. But many grants are approved for funding and not funded. That's true of all funding agencies.
MR. SCHROEDER: Mark that.
BY MR. BERNICK:
Q Dr. Burns, I'm not asking you about all funding agencies.
A I don't have a specific instance of mind of a specific grant.
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Q Are you aware of any group of grants or any category of grants that the SAB approved for funding but that were not ultimately funded by the CTR?
A Independent of the grants on central nervous system?
Q Yes, separate -- setting aside central nervous system.
A Okay. That is the principal area where I have seen documents that the funding authority of the SAB was subverted. I don't -- or funding approval of SAB was subverted. I don't have a specific instance other than that of the -- of documents that describe a subversion of the SAB's funding priorities.
MR. SCHROEDER: Mark that.
BY MR. BERNICK:
Q I'll try one more time.
A I told you that I don't have a specific instance in mind. What more do you want?
Q Because I asked you -- you told me specific instance. You told me that twice. I asked you for a group or category or collection of some kind, so that we're not just talking about there's one, or there's another. I'm asking, are you aware of any group or any category, any kind of characterization of grant
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proposals or contract proposals that were approved for funding by the SAB but that ultimately were not funded by the CTR? Setting aside again the research in the late 1970s into central nervous system effects of smoking.
A It is my understanding that the process of the research funding was such that grants were not presented to the -- please let me finish. Were not presented to the CTR board and then not available for funding. And therefore, I'm not aware of a body of grants that had been presented to the CTR board and not funded.
Q I --
MR. GRUENLOH: Hold on a second.
MR. BERNICK: At this point --
MR. GRUENLOH: Mr. Bernick, hold on a second.
MR. BERNICK: I'm not going to ask him a question.

MR. GRUENLOH: No, hold on one second.
MR. BERNICK: I'm not going to hold on. If you are going to interrupt the deposition, we will end the deposition right now.
MR. GRUENLOH: Then let's go off the record.
MR. BERNICK: No, we're not going to go off the record.
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MR. GRUENLOH: The faces that you're making, the little sighs, the laughs, this is disrespectful. And we will discontinue this deposition at this point if it continues.
MR. BERNICK: I'm expressing at this point frustration of the witness' refusal to answer the question. I'm going to discontinue this line of examination. This will be the second line of examination at the very least that we're going to raise with the judge.
MR. GRUENLOH: It's the same line of examination that you said you were going to discontinue before.
MR. BERNICK: No.
MR. GRUENLOH: You never left it.
MR. BERNICK: It's a completely different one.
Q Are you aware of any evidence that any member of the SAB ever acted in bad faith?
A Could you define for me what you mean by bad faith in relation to membership in the SAB?
Q That they knowingly failed to do their job, that they knowingly falsified recommendations or approvals, anything that would mean that they are knowingly failing to do their job as scientists and as
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members of the SAB.
A It is my understanding that the scientists on the SAB fulfilled the responsibilities they were asked to fulfill by the TIRC in terms of reviewing and approving grants. It is my understanding that they tended to be somewhat biased to their own research and their own institutions in terms of those grant approvals, but that that was not something that was of substantive concern to the CTR and was not considered to be an inappropriate or irresponsible action on the part of those members.
Q Are you aware of any research sponsored by CTR that was falsified?
A I don't have a specific recall of all of the research funded by CTR. I am not aware of research that was published where the data was deliberately altered in publication.
MR. SCHROEDER: Would you mark that one for me, please?
BY MR. BERNICK:
Q Are you aware of any research that was sponsored by CTR where the publication of the research was knowingly altered or changed in any way so as not to accurately reflect the results of the research?
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A It is my understanding that there are at least two instances where that has come into question. One

was the work by Dr. Humberger (phonetic), Freddie Humberger, where he was asked to change the description of his work as it appeared in publication.

Q Did he?

A My understanding is that he modified it somewhat, but that ultimately he did not agree to do that.

The second example was in work that has been funded, I believe, on a contract where the examination of animals over a long period of time was terminated, and a very long, detailed report of that was written that one of the principal investigators of that effort feels did not reflect the body of information that that research had demonstrated.

Q That's the MAI contract?

A It's Dr. Henry, I believe.

Q She worked on the MAI contract?

A I'm simply giving you the information I have, and not trying to contradict you.

Q Isn't it true that Dr. Henry wrote the article that came out in the Journal of National Cancer Institute reflecting the results of that work?

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A I am not specifically conversant with that article. I'm referring to a larger report on that body of work.

Q The larger report is called the Blue Book?

A That's correct.

Q Isn't it true that the Blue book was written by Dr. Henry and Dr. Curry?

A I don't believe that they felt that the final draft of that version reflected their initial input. At least as I recall from having read Dr. Henry's deposition.

Q Are you really familiar enough with that contract to express expert opinions regarding falsification of research or improper statements of research, Dr. Burns?

MR. GRUENLOH: Object to form.

BY MR. BERNICK:

Q I'm just asking you.

A You had asked me for examples. I tried to give you an example. I'm certainly expert enough in the conduct of research and those areas of research to review that in more detail and offer more detailed comments on it.

Q At this point in time have you done work

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sufficient to offer expert opinions regarding the conduct of research by MAI?

MR. GRUENLOH: Same objection.

THE WITNESS: I have done sufficient work to believe that the conduct of that research, okay, resulted in a document being produced that I believe is not consistent with the internal data contained, and that I believe the -- one of the principal investigators has characterized as not being a scientifically valid expression of that work. I would be in a position to offer that opinion. If it is an issue that requires more detailed discussion, then I would obviously need a more detailed review to provide that detailed

discussion.
BY MR. BERNICK:
Q Dr. Burns, are you aware of any medical association or public health authority which has relied upon statements made on behalf of CTR in the conduct of -- strike that. In developing policy concerning smoking and health?
A I honestly don't know -- I know that the presentations made by tobacco industry spokespersons commonly cite the CTR. I don't believe that they use direct expressions from the CTR in those presentations.
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I am unaware of direct presentations by the CTR that have influenced those positions.
Q Let me ask you just one more question, or a couple more, and then we'll take a break for lunch.
A Okay.
Q If that's all right with you.
A That would be wonderful.
Q You're familiar with Dr. Benowitz, are you not?
A I am.
Q He's an authority in the field of smoking behavior; correct?
A He's authority specifically in the area of nicotine pharmacology and its impact on smoking behavior, yes.
Q If I were to tell you that Dr. Benowitz has testified that the pharmacological research done on nicotine under contract with Batco in the early 1960s was not novel research, would you have a basis as an expert to say that he was wrong?
A Without reviewing the context of that statement, I wouldn't have a basis to say that he was correct or incorrect.
Q If I were to tell you that Dr. Benowitz has said that research would not have affected the
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conclusion reached by the Surgeon General in 1964 that smoking was a habit, again would you be in a position to say that he was wrong?
A I would agree with him on that, as the characterization that was used in the Surgeon General report was based on the WHO criteria which required social deviancy, and therefore, the pharmacological basis for nicotine addition would not have influenced that conclusion.
Q I want to show you Dr. Benowitz' testimony in January of this year. This is his deposition in the National Asbestos Workers case at pages 131 and 132. And I've highlighted beginning at line 15 on 131 and going over the discussion regarding the Project Hippo work.
A Okay. I guess I --
Q Do you want to wait until I give you a question?
A No, I'm just a little confused by some of the language here. But let me get through it.
Q Okay.
A Okay.
Q Do you see where he says down at the bottom of the first page he's asked whether the Hippo --

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A Do you want me to hand this to you or do you want me to see it?
Q Why don't you just keep it.
A Okay.
Q Do you see where Dr. Benowitz is asked whether his opinions regarding the novelty of the Hippo research have changed? And he says they have not.
A I believe the question you're referring to is, "So the notion that there was nothing new or anything novel in Project Hippo in 1963 would certainly be true in 1976; isn't that right"?
That was the question I had some difficulty understanding.
And Dr. Benowitz answered yes to that.
Q Would you take a look at the prior page?
A I did.
Q He's asked in the prior page whether he regards -- whether he still is of the view that the Project Hippo work was not novel when it was done in the early 1960s. Do you see that?
A Okay. He says -- the question is, "Do you recall whether or not you agreed with Mr. Bernick that there wasn't any real new data or any new ideas that were reflected in the Project Hippo report? To use your
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words, that there was nothing novel?"
Ms. McDevitt: "Objection."
The witness: "I don't recall those words specifically, but I shouldn't ... I could have said that."
"Do you have any basis to disagree with that sitting here today?"
"No."
Q Do you have any reason as an expert to disagree with Dr. Benowitz' testimony, as you have now recited it, on the novelty of the Hippo work?
A I don't believe that I have the context of Dr. Benowitz' testimony. I don't know what specifically he is referring to, and therefore, I'm not in a position to offer an opinion one way or the other as to whether I would agree with his interpretation of that.
Q Have you made a review, Dr. Burns, of the literature prior to 1964 on the pharmacology of nicotine?
A I have generally reviewed that information. I have not conducted a specific review of that information for purposes of this trial or this deposition.
Q What in the Hippo work that was done before 1964, what in the Hippo work was novel; that is, did not
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appear in the then-existing literature on nicotine pharmacology?
A I would have to go back and review that body of work to offer you a reasoned answer to that question. I have not conducted a review of that work or of the literature at that time for purposes of answering that question for this trial, and therefore, I don't have a basis to answer your question.
MR. BERNICK: Let's take a break for lunch.

THE VIDEOGRAPHER: Off the record at 12:47 p.m.
(Lunch recess.)
THE VIDEOGRAPHER: This marks the beginning of Videotape Number 3 of Volume 2 in the deposition of Dr. David Burns. We are back on the record at 1:28 p.m.
BY MR. BERNICK:
Q Dr. Burns, in 1988 the Surgeon General issued a report concluding that nicotine -- or that smoking is addictive, and that nicotine in particular is addictive; correct?
A That's correct.
Q In so concluding, the '88 report adopts the criteria for dependence that were then in use through organizations such as the APA; correct?
A Yes. Among others.

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Q Would it be fair to say that the criteria that were applied by the Surgeon General in '88 in concluding that smoking was addictive are broad enough so that they would also require a finding that certain people who consumed coffee are addicted to caffeine?
A I think that there are aspects of those descriptions that could be applied to coffee, that's correct.
Q And in particular, Dr. Benowitz, Dr. Honeyfield, and others have done the analysis and have concluded in writing that caffeine can be addictive; true?
A That is correct. And that is consistent with others who have done the same thing.
Q In fact, isn't it true that Dr. Benowitz petitioned the Food and Drug Administration to conduct research into caffeine because, precisely because in certain circumstances caffeine could be addictive?
A I have no basis to know whether he has or has not done that.
Q Not from the article that Dr. Benowitz has published on caffeine?
A I've not read that article, no.
Q Are you an expert in the field of substance

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dependence beyond the work that you've done in particular in connection with tobacco?
A I don't know what you mean. I have a substantial background in substance dependence as part of my training in medicine. I have a particular training in substance dependence because of my background in intensive care medicine. And I have a particular interest in one form of substance dependence that I have pursued over the last 25 years, which is tobacco.
I have at various points in time participated in activities related to substance dependence in other areas, but it has principally been focused on tobacco.
Q You have -- would it be fair to say, I think, you've testified previously that the term "addiction" has different meanings to different people?
A I think that's true.
Q I'm sorry?
A I said, I think that's true.
Q Okay. And you've also described your own

definition of addiction, have you not?
A Yes, I have.
Q And just so that we can have a point of reference, you've said that -- you use the term
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"addiction" for those things that interfere with someone's ability to make choices about their future behavior and that have a negative or detrimental effect?
A That's correct.
Q With that definition of addiction, people who overeat could fairly be described as being addicted to eating; correct?
A That's a much more complex scenario, because it is not possible to simply stop eating. And the aspect of the behavior that influences overweight has to do with both control of impulses and also influences of hunger. And so it would be a much more complex thing to describe simply with addiction. But certainly people who are overweight where their overweight is detrimental to their health and they are unable to alter behavior in a way that reduces that when they are actively trying to do it, I think it would meet that definition, sure.
Q What?
A It would meet that definition under those circumstances, yes.
Q People that continue to jog --
A Let me make it clear. People would not say they're addicted to food. They would say that they have an eating disorder.
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Q But they would satisfy the definition of addiction that you have?
A Well, they -- it's more complicated because of the fact that you're not describing a use of a substance, because everyone needs to eat. What you're describing is an aberration in that behavior beyond the level of ingestion of nutrition necessary to maintain body weight. Therefore, it's a more complex description than can be simply, "You're addicted to food." You are -- you have aspects of the behavior of eating that are abnormal and consistent with a compulsive behavior, which is the characteristic that has been associated with addiction.
Q All I really want to know is that for people who overeat and persistent overeat even when -- overeating when they're told it's detrimental to their health, would those people, or would they not, satisfy the definition of addiction that you have described?
A There are elements of that description which they would satisfy. It's a more complex issue because one cannot say they're addicted to food because everyone eats.
Q People who gamble to the point that they lose too much money and it affects their lifestyle, could it
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be said that those people are addicted to gambling?
A It can be said that those people are addicted to gambling. It is said that those people are addicted to gambling. In that setting one is talking about a behavior and an addiction to a behavioral response or

reaction to a certain circumstance rather than to a pharmacologic agent. But one can characterize the resultant behavior with the same terminology, and many people have.

Q People who consistently drive cars too fast and engage in reckless driving because it provides them with a thrill, is fast driving addictive to those people?

A I can't tell you whether it is or isn't. I've not examined those -- the evidence on those folks enough to know. I've not seen that described as an addiction. I think it would be very difficult to qualify that as a medical illness, that certainly that would require any kind of accommodation.

Q People who jog even after they've been told by their doctors that it's hurting their joints, are they addicted to jogging, within your definition?

A For people that compulsively jog when they know that it is injuring them and are unable to stop doing that despite a volitional act on their part to stop, I

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think would meet that criteria. Again that's not something they're addicted to an agent, but rather to the response to a behavior.

Q Dr. Benowitz has testified that he doesn't use the word "addictive" before a jury because it's a loaded term. Would you agree with that?

A I think that addiction carries with it a number of unattractive connotations. It also carries with it in the general understanding of that term a strength of the association or strength of the limitation of behavior, both of which are less with other terms that have been used, such as dependence.

Q Dr. Benowitz has also said that he does not tell his patients, that is, his patients who are smokers, that they are addicted because he doesn't think it helps them. Would you agree with that?

A Would I agree that he does that?

Q No, would you agree that telling smokers who are being seen for smoking cessation, telling them they're addicted doesn't help?

A I think that most smokers acknowledge that they're addicted. My experience with people has been that that is a term that they use about their own behavior. That is a term that I have used in talking

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with patients. I don't believe that emphasizing the dependence, per se, is particularly helpful in interacting with a patient. I think what you emphasize are the tools necessary to break the dependence, or break the addiction, rather than reinforcing whether or not they're addicted. But I certainly have used that term with patients.

Q Is Dr. Benowitz' approach a reasonable one, which is to avoid using the label "addiction" in front of juries and to avoid using the label "addiction" in dealing with smoker patients?

A I think that that depends on the context in which one is speaking to juries and patients. I would think that Dr. Benowitz would be unlikely to tell a patient who said, "Are cigarettes addicting?" "No." I think he would say, "Yes, that they are addicting."

I think what he is saying is that he does not feel it is terribly effective to focus on the presence of dependence, presence of the addiction as a principal characteristic of interacting with people to get them to quit.

And he uses the term "dependence," because that is the one that has been used in the general scientific literature, particularly that scientific literature that

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Dr. Benowitz has published in. Both of those are reasonable positions.

Q There are a number of people who do specific scientific research in the area of smoking behavior; correct?

A That's correct.

Q Are you familiar with people who are recognized experts within the field of smoking behavior?

A I know some of those people.

Q Is Carl Babbitt a recognized researcher in the field of smoking behavior, out of Germany?

A I can't tell you. He's not someone that I am familiar with.

Q Have you taken a look at the literature, the current literature to see what people who are specialists in smoking behavior are saying today about whether the proper term in describing smoking behavior is "habit" or whether the proper term is "addiction"? Have you looked at that literature?

A I have looked at that literature. My understanding is that the current use is to use the term "dependence."

Q Okay. Are you aware, though -- have you taken a look to see in the literature whether there has been

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discussion about the advisability of using the term "addiction"?

A I'm sure that somewhere in the literature there have been discussion of those issues. There was certainly a discussion of that issue around the '88 Surgeon General report. I'm not conversant with a specific article. If you have something in mind that you want to show me, I'd be happy to look at it.

Q No, I'm really just kind of asking whether you've kept up to date on what people in the field of smoking behavior are saying is proper terminology in talking about smoking behavior, whether it's dependence, or addiction, or habit? Have you just kept abreast of the literature?

A I am generally conversant with that literature. I'm not specifically conversant with a document that you may or may not have. It is my understanding that the people who work in that area currently use both the terms "addiction" and "dependence."

Q Are there others who also use the term "habit"?

A I am not specifically conversant with people who use the term "habit" in an effort to characterize the relationships of ingestion of a drug in a compulsive way that leads to alterations in their choices about

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subsequent behavior.

Q I'm going to show you an article that was published by Dr. Babbett in 1998. And direct your attention to a paragraph that I've highlighted for you. And ask you to read that for a minute.
A Do you have the article? I mean, you have given me page 83 and page 110 and 11 out of an article that is from page 83 to 115.
Q Right, I've just given you an excerpt.
A Okay. I have the excerpt.
Q All you have is the excerpt, that's correct.
A Okay. What would you like me to do with it?
Q If you would just take a look at the paragraph that I have marked there. And just read that over, if you would.
A I have read the excerpts. Do you want me to hand it back? Okay.
Q For the record, it says, quote, "Nevertheless, it is still a controversial issue as to whether smoking should be considered as an addiction rather than as a, perhaps, strong habit. This issue was discussed by several authors among other occasions in a special issue of Psychopharmacology in 1992 and also at the occasion of an international symposium on nicotine." And there's
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a 1994 citation.
"These discussions suffer not only from the fact that the differences between heavy smoking and decompensated needs for alcohol, opiates or cocaine are profound. Another basic difficulty is there's no generally-accepted definition of addiction."
Is it a reasonable statement for a scientist within the field of smoking behavior today to make the statement that says that, "It is still a controversial issue as to whether smoking should be considered as an addiction rather than as a, perhaps, strong habit"?
A I think that that is not consistent with the mainstream of scientific thought. I also think that the opening sentence of his article is also not factually accurate. The opening sentence of his article is that --
Q I didn't ask you about the opening sentence of the article, Dr. Burns, did I?
A You're asking me for my opinion as to whether this gentleman's positions are consistent with those of the scientific community.
Q No, I asked you a very specific question, and you still haven't answered the specific question.
A Well, ask your question again.
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MR. BERNICK: May I have the question read back?
(Record read as follows: "Is it a reasonable statement for a scientist within the field of smoking behavior today to make the statement that says that, 'It is still a controversial issue as to whether smoking should be considered as an addiction rather than as a, perhaps, strong habit?'")
BY MR. BERNICK:
Q Can you answer that question, please?

A I think that it is difficult for me to offer an opinion about whether that is a reasonable statement in the absence of reviewing the discussion that he describes prior to the statement "nevertheless." Okay? I think that that is not an accurate characterization of current scientific thinking. And the limited amount of information I have on this article that you have given me would lead me to believe that other statements in the article, particularly the initial sentence, are also not consistent with my understanding of the scientific position at this point in time.

Q So in your view there is no -- it's not

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possible and reasonable for a scientist in the field of smoking behavior to say that there's still controversy over whether smoking is an addiction or habit? It's no longer controversial?

A I think, as I've said, that without reviewing what this gentleman is referring to as his discussion of the data and of the definitions he's choosing to use for addiction and habit, it is very difficult for me to offer a position, okay, as to whether or not that is a reasonable statement or not.

In science there are always issues that remain controversial as you move forward. That is the nature of science in a continued quest for information. As to whether or not the principal characteristics that I have defined as addiction are generally accepted in the scientific community as being consistent with the use of the term "addiction" and whether those characteristics relate to the nicotine in cigarettes, I think that those issues are no longer controversial within the scientific community.

Without knowing the specifics of how this gentleman is choosing to use the word "habit" as opposed to "addiction," then it makes it very difficult for me to offer an opinion as to whether he is expressing a

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reasonable position or not.

MR. SCHROEDER: Mark that.

BY MR. BERNICK:

Q Would you agree that it is controversial today -- strike that.

Would you agree that there is no generally accepted definition of addiction today?

A No, I would not.

Q Would you agree that there is controversy, given what Dr. Benowitz testified to, as to whether smoking should be described as addictive versus being described as dependence producing?

A I think that there is a difference in choice of use of those terms amongst the scientific community with the preference of those people who are working in the field being for the use of the term "dependence."

Q Dr. Burns, Dr. Benowitz also has stated -- could you hand back the first part of that article to me, please? Thank you.

Dr. Benowitz has also said what matters is not the label that is applied to smoking behavior, that is, whether it's addictive or not. What matters is conveying the fact that it can be difficult to quit

smoking. Would you agree with that?
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A I would agree, depending on the context in which it was written, that that is important. I believe it is also important to define whether or not it meets the characteristics of addiction or characteristics of dependence for a variety of informational and public policy purposes. But in general, I think that the principal -- I would agree with Dr. Benowitz that the principal issue is whether -- is not the term that is used but the impact of that ingestion on the behavior of the individual.
Q In other words, what's important to convey to people is not necessarily the label, but the ultimate fact that it can be very difficult to quit smoking; would you agree with that?
A Well, I think that it's slightly beyond that. It's important to convey to them that the reason why it is difficult for them to quit has to do with their need to compulsively ingest a given substance, and that that substance is a principal part of the reason why they're having difficulty quitting.
Q And that substance being nicotine?
A Nicotine, that's correct.
Q And recognizing that the label that's used in talking about smoking behavior is much less important
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than the operative fact that people have a hard time quitting because of nicotine; is that fair?
A The behavior consequence is clearly the most significant issue. The choice of a word to communicate that is dependent on the need to communicate accurately the reality and the strength of the change in behavior that accrues from repetitive injection of nicotine through cigarettes. And that, therefore, the choice of the term that is used is an effort to communicate accurately what the consequences of that regular ingestion of nicotine through cigarettes will be or is.
Q And would you agree that in Dr. Benowitz' view it is less productive to use the term "addiction" and more productive to use the word "dependence"?
A No, I would not agree with that as a global statement. I think Dr. Benowitz may have expressed that in certain contexts. Dr. Benowitz was also one of the editors of the Surgeon General's report where they chose to use the word "addiction." So I would expect that his considerations of those issues would not be as global as your question would imply.
Q Dr. Benowitz has also testified that the essence of the definition of "addiction" is "difficult to quit." Would you agree with that?
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A Yes. I think that the most important characteristic of that definition is the interference with the choice to stop smoking.
Q And isn't it true that that property of smoking, that is, the addictive effect of smoking in the sense that it's hard for people to quit, has been known to the scientific and medical community prior to 1964?
A I think that it has been known for some time

prior to 1964 that people have difficulty stopping smoking, that is correct.

Q Indeed, isn't it true that according to Dr. Honeyfield -- he's another specialist in this area, is he not?

A Yes, he is.

Q I want to read from you -- read to you a statement that he makes in an article that he published in 1988, where he said, "Centuries ago the answer was known tobacco can addict those people who sample it and it can addict with the power of substances such as alcohol and opium." Would you agree with that statement?

A Would I agree with the statement that nicotine can addict and that that has been -- that people have known for quite some time that use of tobacco creates a

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difficulty in stopping the use of tobacco? Yes, I would agree with both of those statements.

I would disagree, however, with the statement that the use of that term in relation to the pharmacology of nicotine, okay, has been well understood for a very long period of time.

Q As to this statement, I'll read it to you again, Dr. Honeyfield says, the statement that's right in front of you, that for centuries it's been known -- the addictive qualities of smoking or tobacco have been known. Would you agree with that?

A I would agree with the statement that people have known that the regular ingestion of tobacco leads to a circumstance where it is difficult to stop that regular ingestion. I would disagree that people have known that the characteristic that drives that is the pharmacologic response and physiologic changes that accrue due to the ingestion of nicotine that we currently characterize with the term "addiction."

So I would agree that people understood the consequences relative to behavior. I would disagree that they knew the specifics of the use of the term "addiction" relative to the ingestion of the nicotine and tobacco.

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MR. SCHROEDER: Would you mark that one, please?

BY MR. BERNICK:

Q And with respect to just practical wisdom on quitting smoking, practical wisdom on quitting smoking that is reflected in the term "addiction," the practical wisdom is the difficulty of quitting; correct? We've already been through this; right?

A The practical wisdom being what?

Q The practical wisdom that is embedded in the idea that smoking addictive is that it can be very hard to quit; correct?

A The practical reason, as I understand it, for the use of that term by the general population is that it is a drug ingested that makes it very difficult to stop ingesting the drug. That's how most people understand that. They currently understand that when they're addicted, they're addicted to the drug nicotine.

Q Isn't it true that the difficulty of quitting

smoking has been written about in the lay press for literally centuries?
A Yes, that's true.
Q Isn't it also true that the idea that people who smoke are smoking for nicotine has also been
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published in the lay press going back into the early part of this century?
A There has been a publication of that information for a substantial period of time, that's correct.
Q Isn't it also true that beginning in the early 1940s -- actually, beginning even before the 1940s, there were scientific publications where scientists said that nicotine was the most important factor in the smoking experience?
A I think that that's also true.
Q And isn't it true that by 1942 scientists were publishing articles that said that not only is nicotine important for the smoking experience, but people smoke for nicotine, like drug users take drugs for the pharmacological effects of drugs; isn't that true?
A There were individual articles that presented that position.
Q And in point of fact, the tobacco industry sponsored articles that were published even before 1964 which said that smoking is addictive; true?
A I don't have a specific instance in mind. I don't know what you're referring to.
Q Did you ever look to see whether -- what
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research the tobacco industry sponsored about the addictive effects of nicotine prior to 1964?
A I have reviewed some of that literature. I don't know specifically what you're referring to. If you have a specific citation, I would be happy to look at it and offer my opinion on it.
Q Is it true that if the criteria applied in the 1964 Surgeon General's report for determining whether smoking was addictive or habituating were applied today based upon today's scientific knowledge, that the answer would still be that smoking is a habit and not an addiction?
A Those criteria applied today would lead to the conclusion that it was a habit. The reason for that is that the criteria used at that time by the Surgeon General's report required evidence of social deviancy in order to differentiate between a habit and an addiction. And there is not evidence of social deviancy, criminal behavior, robberies, assaults and other kinds of activities in relation to cigarette smoking. That is a criteria that was disregarded shortly thereafter.
Q One of the other criteria that was applied in 1964 were the intoxicating qualities; correct?
A That's correct.
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Q And isn't it true that both then and now there is not evidence that nicotine is intoxicating?
A I think that depends on your definition of the term "intoxicating." If you are using the term

"intoxicating" in its more traditional sense of interfering with judgment, there is no evidence that nicotine is intoxicating. If you use it in a broader sense of having a psychoactive effect, there is evidence that nicotine does have a psychoactive effect.

Q How was the Surgeon General in 1964 using the term "intoxicating"?

A I believe he was using it in terms of it creating a euphoric effect that interfered with judgment.

Q And again today -- based on today's science, again that criteria would not be satisfied by smoking; correct?

A That's correct.

Q The Surgeon General also talked about physiological withdrawal being another criteria for addiction in 1964; true?

A That's correct.

Q And again, by today's science that criteria would also not be satisfied by smoking?

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A I don't believe that that is true. I think that there is evidence of physiologic withdrawal from nicotine that would meet that criteria.

Q Is there any evidence that you're aware of, in the sense scientific studies -- are aware of any scientific studies which demonstrate that people are more able to quit smoking once they know that cigarette smoking is addictive?

A I'm not aware of a study that has investigated that point one way or the other.

Q Are you aware of a study which has investigated whether people are less apt to start smoking if they heard that smoking is addictive?

A I'm not aware of a study that has examined that particular issue in isolation. It certainly is an issue that is of concern in presenting information to adolescents. But I'm not aware of a study that has examined the effect of providing that information in isolation of the other information that is provided to adolescents in an effort to help them resist starting to smoke.

Q Are you aware of the degree -- strike that.

Are you aware of any studies which establish that people today are better informed about the

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difficulty of quitting smoking than they were at any point in time in the past?

A I'm not sure what you're saying.

Q I'll withdraw the question and make it clearer.

A Okay.

Q We've talked about the fact that the practical wisdom about the term "addiction" applied to smoking is the difficulty of quitting?

A Yes.

Q And all I'm asking is, have any studies been done to determine whether people today are more or less aware of the fact that it's tough to quit smoking than they have been in the past?

A It is my understanding that there have been survey that has been done that has shown an increase in

the frequency in which people report positive responses as to whether smoking is addictive, and also positive responses on the part of smokers as to whether they're addicted.

I'm not aware of an increase in survey work that has shown, or a change in survey responses to the question, "Is it difficult to stop smoking?"

Q Let me ask you a few questions about another subject, and I will pass the baton to somebody else.

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Not, of course, leaving you free to go.

A I had not --

Q We wouldn't want to disappoint your expectation that you're chained to --

A I have not expected --

Q -- your chair for the rest of the day.

A -- that that would happen.

Q All right. Are you aware, Dr. Burns -- you've testified previously -- what did I do with that? You testified previously about the -- whether the FTC delivery method had the effect of deceiving smokers about their exposure to smoke from low delivery cigarettes; do you recall that?

A I don't recall testifying with that specific characterization. I certainly have testified before on the disease consequences of different brands of cigarettes, and on measurement of tar and nicotine of cigarettes of different manufacturing types.

Q But more specifically, I think you've testified about whether people who smoke understand what the FTC delivery figures mean; that is, the tar and nicotine figures mean?

A I have testified on the issue of what people understand from both those figures and also from the

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characterization by brand of terms such as "light" and "utralight."

Q Okay. Are you aware of any new studies that have been done to determine the degree to which the FTC method provides guidance on how much smoke the smoker actually takes in from these different brands? Is there any new science out there on this?

A I'm not sure what you're saying. From what point in time would you like me to characterize the --

Q Since the fall of 1998.

A There is a substantial body of work that we are currently working on which has been presented to the Institute of Medicine and was part of a deposition in the Little case, that has to do with the relationship between disease consequences, specifically lung cancer, and various brands of cigarette characterized by tar. It also has to do with changes in number of cigarettes smoked per day, based on changes in brand proportional to the change in nicotine.

And there is also data on the use of numbers of cigarettes in California based on the tar and nicotine rating of those cigarettes.

In addition, there is work that is in process to collect new survey data by the Robert Wood Johnson

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foundation on the specific issues of what people understand about low tar and nicotine cigarettes.

Q But is there any -- is there any new studies that have been done on what the actual deliveries to the person are versus the machine delivery?

A I'm not sure what specifically you're asking. Certainly the data from California on the number of cigarettes smoked per day relative to the nicotine yield is a substantively new contribution to the area of how much people ingest with brands of cigarettes with different tar and nicotine.

Q Who published that study?

A I am doing that work in conjunction with people in my office. We have presented that work at the Institute of Medicine. It has not yet been published.

Q Let's just focus on published work. Is there any more recent published work on the question of how actual smoking delivery compares to the machine deliveries?

A I would have to go back and look as to whether things have been specifically published from the fall of 1988, is what you're asking me?

Q Right.

A There may be additional studies that have been

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published since that time. I don't have that date fixed firmly enough in my head to tell you whether the studies that I've reviewed are before or after that date.

Q Have you written an article or co-authored an article with Mr. Penny on the question of whether the FTC method of measuring deliveries is -- gives meaningful information? Actually, it's an editorial.

A Is that what you were referring to by the date 1998?

Q No. Since 1998. That's what I was asking.

A Okay.

Q You were involved in writing an editorial, were you not?

A I was involved in writing an editorial on an article that was published in the Journal of the National Cancer Institute. If that's what you're referring to, yes, I believe that that data, as I recall, is after the 1998 date.

Q What's the data that was published in the -- is it Journal of the National Cancer Institute?

A Yes.

Q What was the data that was published about smoke deliveries?

A As I recall, and it would obviously help to

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review the article again, it was an article that examined the machine-measured yields of low tar and nicotine and higher tar and nicotine cigarettes when the machine smoking parameters were set based on the actual observed smoking parameters of individuals who had smoked those cigarettes.

Q Who are the -- who are the researchers in that work?

A I don't remember the specific names from American Health Foundation.

Q You got the lead one mentioned here is

Djordjevic?
A Right.
Q Who is that?
A He's an individual at the American Health Foundation, as far as I understand. I don't know him personally, or her personally.
Q You say that that study, quote, "Elegantly demonstrates that the Federal Trade Commission method of testing cigarettes for tar and nicotine provides tobacco companies the opportunity to mislead their customers."
A That is what was written in the editorial, that is correct.
Q You were one of the authors of that editorial;
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correct?
A I was one of the signatories to that editorial, that is correct.
Q Apart from whether you believe that the Djordjevic, that's D-j-o-r-d-j-e-v-i-c, that that study was elegant or not, is Djordjevic a recognized researcher in this area?
A I believe that he is, he or she is someone who has done work in this area. I don't have a specific knowledge of their CV.
Q Did you take a look at the article published by Djordjevic in the Journal of the National Cancer Institute to see whether it applied sound methods and reached sound conclusions before you signed on to this editorial?
A Yes.
Q What did you find out about the situation?
A I thought it was a credible article.
Q Credible article. I didn't really think I was asking about that. I asked whether the article applied sound methods and reached sound conclusions about the data that it gathered?
A I'm sorry, I had thought that's what was the definition of credible. As far as I understand, it
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applied sound methods and reached sound conclusions.
Q And this is the Djordjevic article that was just published in the Journal of the National Cancer Institute?
A Yes.
Q Okay.
(Discussion off the record.)
(Deposition Exhibit 6 marked.)
BY MR. BERNICK:
Q Is Exhibit 6 a copy of the Djordjevic article that you reviewed and that you've just been referring to as the subject of commentary, the editorial that you helped write?
A As far as I can determine, this is a copy of that article.
Q And did you -- before you again -- this is a fairly strong statement that you made in the editorial, that, "The article elegantly demonstrates that the FTC method provides tobacco companies the opportunity to mislead their customers." It's a fairly strong statement, isn't it, Dr. Burns?
A I think that is a strong statement of what is a

generally-accepted fact at this point in time.
Q Well, did you -- before you wrote the editorial
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that you did this year in the JNCI, did you -- I don't see anywhere in the editorial where you state that there are any limitations or qualifications in the work that Djordjevic did and published in Exhibit 6?
A The fact that one in an editorial doesn't describe every aspect, positive, negative, strength, weakness, limitation, complete citations of a given article, does not necessarily mean that one is either endorsing that or is implying that there are no limitations. The --
Q Well --
A -- purpose of an editorial is to focus on an aspect of the article that is particularly significant, and to help the readers understand the significance of the article. It is not intended usually to be a detailed criticism of all aspects of the article.
Q Well, before you signed on to the editorial, did you -- were there, in fact, limitations or qualifications on the quality of the research that Djordjevic did as published in Exhibit 6?
A As I've testified earlier in this deposition, I know of no scientific article, epidemiologic or otherwise, that does not have limitations. Everything by definition is finite and has limits.
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If you're asking me about specific pieces of methodology that you would like me to comment on, I'd be happy to do that. I'm also happy to acknowledge that this piece of work is not a complete, total description of everything that is ever going to need to be known on this topic. It is a piece of work that describes specific observations under specific circumstances. By definition of everything that we understand in science, that, of course, places limits on the information available and the interpretation of that.
Q Are you aware of any erroneous conclusions that are reached by Djordjevic in his article, Exhibit 6?
A I haven't reviewed the article specifically to attest to whether or not I agree with every single statement contained in the article. If you would like to ask me about a specific statement, I would be perfectly happy to give you my opinion on that. But as I sit here from memory, I'm not capable of recalling every sentence that was written in the article.
Q Do you recall what it is that Djordjevic found about the total exposure to smoke of the groups that he was studying? That's a poor question. I'll withdraw.
A Yes, it's a poor question.
Q He was taking a look at people who smoked
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medium tar cigarettes and low tar cigarettes; is that correct?
A That's correct.
Q Do you recall what he concluded about the level of the smoke exposure as between the two groups?
A He has a series of very specific conclusions that he reached. And they relate to both the ingestion

of tar and the ingestion of some of the individual chemical constituents. And his conclusion is that the FTC protocol underestimates nicotine in carcinogen doses to smoker and overestimates the proportional benefit of low yield cigarette.

Q Do you agree or disagree with that?

A Agree with that statement.

Q Okay. Go on.

A "Thus, FTC-based nicotine medication doses prescribed/recommended for smoking cessation may need to be reassessed."

Q Do you see where he makes a statement -- if you'll hand me, I'll show you more precisely. I'll mark it with a dot.

A Okay.

Q Where he reaches the conclusion that smokers of medium yield cigarettes compared with smokers of low

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yield cigarettes received higher doses of all components?

A That is what he concluded, that's correct.

Q Do you have any reason to question that conclusion?

A No. I think that the conclusion is substantiated by the data that he presented.

Q I want you to direct your attention, if you would, to --

A Perhaps I should caution you, however, so I don't leave you in the lurch. That is not a statement that says people who switch from one type to another will receive lower doses. That is simply a statement that the population of people who use these products, okay, is a different population of individuals who ingest less nicotine and, correspondingly, ingest less tar.

Q Do you know whether or not the population that he is looking at includes switchers?

A I am sure that the population he looked at includes people who have switched. The population he looked at, as I recall, and if we need to explore the issue in detail, we should go back and look at the article, was individuals who were using those brands

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stably, rather than individuals who were specifically switching from one brand to another.

Q But if you wanted to see whether switching down, that is, switching from a higher delivery cigarette to a lower delivery cigarette, ended up reducing total smoke intake, you want to look at that reliably, you'd have to take a look at the results of the switching over a long period of time, would you not?

A In order to examine that question, one would have to examine that in a variety of contexts. It's been a complex question to examine. We know that there are differences in the use of the first cigarette, that do not persist over multiple cigarettes for the next days. We know that there are differences that are present in the first days that are not present over a longer interval.

We know there are differences that are observed when you ask people to switch that are not observed in

people who spontaneously switch. We know there are differences in people who switch because they are trying to quit, in comparison to individuals who switch without an effort to quit.

There's a variety of differences that need to be accounted for in examining the question of to what

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the actual effect is when one switches from one brand or the other.

Perhaps we could take a short biologic break. I will be back in a minute or two.

MR. BERNICK: Okay.

THE VIDEOGRAPHER: Off the record at 2:19 p.m.

(Recess.)

THE VIDEOGRAPHER: We are back on the record at 2:23 p.m.

BY MR. BERNICK:

Q Dr. Burns, the FTC has regulated cigarette advertising since the late 1930s; correct?

A I'm not sure exactly what you're saying. The FTC is certainly responsible for investigating false and misleading advertising, and certainly has interacted with the tobacco companies episodically over that period of time on various issues. They do not, in fact, actually regulate the content, quality or any other aspect of the tobacco advising, per se, independent of its false and misleading nature.

Q That's the point, is the FTC has got the power of the federal government to monitor cigarette advertising. And if they feel that the cigarette advertising is false or deceptive, they have the

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authority to act to cause that advertising to cease; correct?

A That is correct.

Q And they have had that authority since the 1930s; correct?

A I don't know when the FTC act was passed.

Q Isn't it true that in 1955 the FTC actually issued specific guidelines pursuant to its power to regulate cigarette advertising to ensure that it wasn't deceptive?

A I'm not sure what you're specifically referring to. They had developed proposed guidelines. I'm not aware if those guidelines were ever implemented.

Q You're not aware of any guidelines the FTC has ever issued regarding claims that are made with regard to the properties of cigarettes in advertising of guidelines?

A No. They have issued a variety of guidelines. I'm not aware of the specific ones you're referring to. My understanding was that during the early stages of the FTC's consideration of this issue they were proposing fairly stringent guidelines on tobacco industry advertising, and that the tobacco industry's response was to develop voluntary guidelines. But if that is not

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the topic that you are referring to, perhaps you could explain it to me.

Q Do you recall in the early 1950s the cigarette

companies began to put filters on their cigarettes and to make claims about the tar and nicotine deliveries of the new cigarettes?

A That's correct.

Q Do you recall that in about 1955 the FTC issued regulations on what the industry could claim with respect to tar and nicotine delivery?

A Yes, they did.

Q Do you recall that during the latter part of the 1950s the FTC took action to stop the cigarette companies from disclosing tar and nicotine deliveries on their packages?

A They prohibited, as I recall, the disclosure of tar and nicotine on the packages because they felt that that was a deceptive form of advertising at that time.

Q And do you recall also that in about the mid 1960s the FTC changed its mind and decided that it would be permissible for the cigarette companies to put the tar and nicotine deliveries on their packages provided that those deliveries were measured in accordance with a method that have been developed by the FTC?

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A They did, indeed, allow that. I'm not sure changing their mind is the appropriate characterization. They felt that it was consistent with what the scientific community was thinking at that point in time, that a method that allowed a comparison between cigarettes in terms of the likely delivery of those cigarettes was something that might be useful to consumers in terms of information that they would use to choose between cigarettes.

Q And the protocol that they required be followed, that is, the method that they required be followed in measuring tar and nicotine deliveries, was a method that the government itself had developed; correct?

A My understanding is that it was developed in conjunction with tobacco -- scientists and tobacco industry scientists, and had been modified from previous methods rather than invented out of whole cloth. But it certainly was one that was specified by the government, yes.

Q Do you recall that at about the time that the FTC determined in the mid 1960s that it would be permissible to disclose tar and nicotine deliveries, that the public health community affirmatively endorsed

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the idea that the tar and nicotine deliveries should be stated by the tobacco companies?

A The public health community on multiple occasions has endorsed the fact, or endorsed the proposition that reducing the amount of smoke, or tar as a measure of that smoke, delivered to people would be something that would be a positive health outcome.

Q My precise question, though, was whether, in fact, the public health community in the mid 1960s formally endorsed the idea that the cigarette companies should disclose the tar and nicotine deliveries of their cigarettes?

A They formally endorsed the concept that the cigarette companies would disclose the delivery of tar

and nicotine to people who smoked those cigarettes.
Q In fact --
A And they endorsed the erroneous concept, as it turns out, that the FTC method would distinguish that.
Q Dr. Burns, in fact, wasn't it true that it was the public health community that actually wrote the FTC and urged them to allow the cigarette companies to disclose tar and nicotine deliveries?
A Since there is no public health community, per se, as an a organized group --
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Q The American Cancer Society.
A The American cancer Society and other individuals, as well as other organizations, clearly endorsed the FTC allowing that information to be present on advertising and endorsed the FTC method as a means of measuring that.
Q Isn't it true that by the end of the 1960s, both the FTC and public health organizations, such as the American Cancer Society, not only wanted the cigarette companies to disclose the tar and nicotine deliveries on the packages, but they wanted the companies to be required to state the tar and nicotine deliveries in cigarette advertising?
A I believe that that is probably true. I don't have a specific recall of that detail.
Q Do you recall that ultimately it became mandatory, that is, Congress passed a law that required the tobacco companies to disclose tar and nicotine deliveries per the FTC method in all of its advertising?
A Yes, I believe that's true. I don't recall specifically that it was a law, but I do know that it was mandated.
Q That's remained in place ever since; correct?
A I believe so.
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Q Has it also remained a requirement on the tobacco companies that they disclose the tar and nicotine deliveries on all the packages?
A I don't know the details of the requirements for the packages. My recollection is that it was required -- that it is on some packages, but not on others. But I don't have a specific recall of the details of that regulation.
Q Isn't it also true that even today, if the -- if and when the tobacco industry has to make disclosures of tar and nicotine deliveries to the consumer, that it's required to use the FTC method of delivery as a meter of law?
A Well, your question includes an assumption that is erroneous. The tobacco companies do not report delivery to consumers. They report delivery to a machine. And that is the genesis of the concern. That what the public health community was, indeed, interested in was the delivery to the consumers. What is being reported is the delivery to the machine, that no longer reflects the delivery even relatively to the consumer.
MR. SCHROEDER: Mark that.
BY MR. BERNICK:
Q We'll get to that in a moment. But my question
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really was a very different one.
A Your question included that assumption.
Q It didn't make any assumption.
A I'm sorry, but that -- I think that was there.
Q I don't want to debate the issue. I'll just ask another question, Dr. Burns.
Isn't it true that where the tobacco industry is required to disclose tar and nicotine deliveries, that is, in its advertising at the very least, and you're unsure about the packages where it's required to disclose the tar and nicotine deliveries, that it must disclose those deliveries as measured by the FTC method?
A Yes, the tar and nicotine delivery recorded on the advertising must be that tar and nicotine measured in machine delivery with FTC parameters.
Q Isn't it true that ever since -- even before the FTC announced the requirement that its machine delivery method be used by the industry, even before that, the FTC was specifically told that its machine delivery method did not necessarily reflect smoke exposure to the smoker?
A The FTC was told that it did not reflect the exposure to individual smokers. It was also told the differences between cigarettes with the FTC method were
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likely to reflect differences in consumption of individual smokers that used those brands. So while the absolute measurement did not reflect the dose, that the information between cigarettes was a piece of information that would have been of use to the consumer.
Q Is there anyplace in any submission made by the tobacco industry to the FTC where the representation is made that the FTC method, in fact, will reflect the relative ranking of cigarettes as the consumer is exposed to smoke from those cigarettes?
A I don't know that the specific wording is contained in the tobacco industry submission. I am aware that the reason the FTC allowed that label to be placed on the cigarettes was that the public health community at that time believed that while the absolute level did not correspond to what people would ingest, that the relative levels across cigarettes as measured by the FTC method by machine would translate into relative differences across those brands in the amount ingested by smokers, and therefore, that would provide information that would be of use to the individual smoker in altering their disease risk.
Q Are you --
A That assumption has turned out to be
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inaccurate. But that is what the belief was that supported the use of that information in advertising.
Q Did the tobacco industry ever make that representation to the FTC or to any other governmental authority?
A I don't know specifically whether the tobacco industry ever made that representation. The representations that I have seen from the tobacco industry state that the FTC yield does not reflect the individual ingestion by the smoker.

Q In fact, isn't it true that the tobacco industry has both in published scientific articles and in actual litigation involving the government specifically stated that the FTC measurements may be misleading to the consumer?
A I wouldn't be surprised that they have made that statement. That is a statement that has been accepted by the FTC and accepted by the public health community within the context of the decisions they made for the purposes that I previously described.
Q Isn't it also true that -- strike that.
You in your testimony in the past have from time to time attacked certain advertising or ads that were used by the tobacco industry to advertise low
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delivery cigarettes; true?
A That's a very broad statement. I certainly have been critical of tobacco industry advertising practices relative to low tar and nicotine cigarettes, yes.
Q Are you aware of any ad, any low tar delivery ad that has been attacked by the FTC as being misleading since 1960?
A I would have to go back and look specifically at the FTC actions. I can't tell you specifically whether they have attacked it or not.
Q In fact, they did attack the Barclay ads, did they not?
A They did. That was on a different issue. But, yes, they did attack the Barclay ads.
Q No, they attacked the Barclay, if you recall, see if this refreshes your recollection, they attacked Barclay ads because they represented they only delivered one milligram of tar, and the FTC felt that given the unique design of the filter, the filter actually delivered -- the cigarette actually delivered more tar than that. Do you recall that?
A I'm familiar with that. I'm familiar with the fact that the ventilation in the Barclay was of a
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different character than the ventilation for other ultra low or low yield star cigarettes, and that that was a matter of controversy to the tobacco companies, even though the tobacco companies acknowledged in their submissions that ventilation was the principal method by which tar was reduced.
Q You're going on talking about a bunch of stuff that has nothing to do with the question, Dr. Burns.
A I'm just trying to be responsive to your question.
MR. BERNICK: Just read the question back, please.
(Record read.)
BY MR. BERNICK:
Q It's relatively simple question, Dr. Burns. Do you recall that?
A I recall.
Q Good. Are you aware of any other cigarette ad that has ever been attacked by the FTC for misrepresenting the deliveries of cigarettes to the consumer, for misrepresenting the safety of low delivery

cigarettes, or any other property of low delivery cigarettes?
A My understanding was that there was -- that was
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the basis of the restrictions early on. I'm not sure exactly the date of those restrictions. I believe you asked me to characterize it from 19 --
Q 1960.
A -- 60 on. I don't know specifically when the FTC ruling was in relation to that date. I'm assuming from your facial expression that it was after that, that the ruling preceded that 1960 date. I'm not familiar with another advertising investigation by the FTC specifically directed at the delivery -- the advertised delivery of cigarettes relative to what the consumer would get.
Q A time came when people began to do research into what was called compensation; correct?
A That's correct.
Q What compensation means is that people who switch cigarettes from one delivery to another may change their smoking behavior because they're getting less of what they want out of cigarette, be that nicotine, tar, or some other property; correct?
A Well, that's a cumbersome definition. The definition that I think makes some sense is that people change the way they use a cigarette with a different yield in order to preserve their ingestion of the --
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predominantly nicotine, but other substances in the tobacco, tobacco smoke.
Q Isn't it true that research into compensation began in the late 1960s?
A I don't know when it precisely began, as in when the first article was published. I know that there was not a substantial body of information that allowed us to truly understand the extent of compensation at the time that we did the 1981 Surgeon General's report. But there was a body of literature that existed at that point in time. And so whatever its initial point was, it was. It was prior to that time.
Q Compensation had significance for low delivery cigarettes because if people compensated completely when they switched to low delivery cigarettes, there wouldn't really be a difference that they would get, or benefit that they would get out of switching cigarettes; correct?
A Compensation is an issue for any change in cigarettes.
Q Including that change?
A Including the addition of filters, including the manufacture of cigarettes, such that they produce a low yield, including the use of ventilation to produce a
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lower yield in the cigarettes.
It also is a factor in design of cigarettes relative to whether or not one would add nicotine to the product.
Q Isn't it true that the tobacco industry sponsored the first publications in the scientific

literature on the subject of compensation?
A I don't know who sponsored the first publication. I've already testified that I don't know when the first publication was published or who do it. I also don't know who sponsored it.
Q Are you aware of anything that has been known by the tobacco industry concerning compensation that is not also published in the scientific literature?
A I know that there is a substantial body of internal tobacco industry documents that defined the relationships between design of cigarettes and the yield of those cigarettes when they were smoked as they would be smoked that was not available at that time to the scientific community. Subsequently much of that information has become available both by release of those documents and by replication of some of those experiments in the general scientific literature.
MR. BERNICK: Could you read my question,
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please?
(Record read.)
THE WITNESS: Perhaps you could define the time frame you would like me to address that?
BY MR. BERNICK:
Q Any time.
A I think I've addressed that any time. The issue was one of a time lag. That information was available to the tobacco companies at a point in time at which it was not available to the general medical literature.
Q That --
A If you're asking me now whether there's information that is not known now that the tobacco companies have, I don't have access to the information that is not known now that the tobacco companies have. Therefore, I can't answer your question.
Q My question is much simpler. The tobacco industry does a study internally. Same study is done externally. Same conclusions reached externally. That's what I'm focused on. I'm asking you whether the tobacco industry has known anything about compensation as a result of its internal research which was not also known at the same time to outside scientists as a result
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of their own research?
A Yes. And I have answered that question. The tobacco companies have known the effects of their design on the yield of those products when they were smoked as they knew they would be smoked. They knew that at the time at which they were designing those products, which was well ahead of the time that those products were either on the market or had been tested by independent investigators and published in the literature. So that information was available to the tobacco companies at a time at which it was not available to the general scientific community.
Q You're telling me that the tobacco companies had compensation information that was specific to brands that were not yet on the marketplace?
A I'm telling you that internal review of tobacco industry documents by me includes review of documents

where they describe the design intent and design effects of the changes that they were engineering into cigarettes. Those cigarettes and those engineering changes did indeed come to the marketplace.

Q Do you know that?

A I know that those changes in cigarettes did wind up in cigarettes. I also know they were testing

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brands of cigarette that were already on the marketplace ahead of the time that similar testing was being conducted and published in the scientific community.

Q There are people within the public health community, including the Public Health Service, that have endorsed general reduction, that is, the reduction of smoke delivery, since the mid 1960s; correct?

A You'd have to be more precise in your time frame.

Q The Public Health Service in 1966 issued a statement encouraging the tobacco industry to reduce the deliveries of tar and nicotine in its cigarettes; true?

A That is absolutely true, and the encouragement was to reduce the delivery to people.

Q And isn't it true that since that time the policy of encouraging general reduction has been revisited from time to time?

A That's correct as well.

Q And isn't it true that even after it became known that people who switch from high delivery products to low delivery products compensate at least to some extent, even after that became known the public health authorities have still endorsed the general reduction of smoke deliveries by the tobacco industry; correct?

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A The public health community has consistently endorsed the prospect of delivering less tar to the smoker.

Q Now, the concept, the driving idea behind general reduction from the point of view of the public health community was to potentially reduce risk to the smoker; correct?

A Well, it was to reduce disease occurrence in smokers, that's correct.

Q Okay.

A Not risk, but disease occurrence.

Q And over time people have done research on whether lower delivery cigarettes, in fact, carry with them a lower risk of disease to the smoker; true?

A Those studies have been conducted, that's correct.

Q And almost since the mid 1960s, the scientists and public health authorities reviewing the data have found evidence to support the idea that lower delivery means lower risk, at least insofar as filter cigarettes are concerned; correct?

A They have found evidence to support the concept that lower delivery to people would result in lower risk.

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Q And that, in fact, the use of filter cigarettes has reduced risk of disease to the consumer; true?

A No. No. That is not true. There was evidence early on that people who used those products had lower risk. We are in process of doing --
Q I'm not -- I'm not talking about what we are in the process of doing, Dr. Burns.
A You're asking for my opinion on the topic.
Q I want the question, I want the same question read back. I'm going to ask you one more time, and this will be yet another subject we have to take up with the judge.
MR. GRUENLOH: You know, don't interrupt him in the middle of his answers. It's rude, it's unprofessional, and you've been doing it for two days. I don't know how you can argue that he's not answering your question when you don't allow him to finish.
MR. BERNICK: Could you please reread the question?
(Record read.)
BY MR. BERNICK:
Q I'm asking you about information that was developed historically, Dr. Burns.
A And I'm trying to tell you what that
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information is, and you don't want to let me finish. Would you like me to describe the information? Would you like me to --
Q I'll make it easier. I'll go back through individual things, and we'll just do the it the slow way. Probably won't have to finish today. Probably have to come back and do some more.
A Well, that would be a matter of some discussion.
Q Well, I'm sorry, but I don't have -- you make me do this. I've got no other choice.
A You won't let me answer your question. I'm perfectly happy to answer your question. You're the one that doesn't want to let me do it.
Q Are you familiar with FTC Monograph 7?
A Yes, I am.
Q That was issued by the National Institute of Health?
A Yes.
Q And that reflected a discussion by Dr. Samet of the question of whether lower delivery products, in fact, reduced the risk of disease to smokers?
A That's correct.
Q And do you remember that Dr. Samet reviewed in
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Monograph Number 7, which came out in 1996, the data that he then had and concluded -- endorsed the conclusion that had previously been reached in the '81 report, that quote, "Today's filtered tip, lower tar and nicotine cigarettes produce lower rates of lung cancer than to their higher tar and nicotine predecessors"?
A That is what he wrote, that's correct.
Q Has Dr. Samet ever changed that conclusion?
A I don't know whether Dr. Samet has changed that conclusion.
Q Dr. Wald reported the study in 1995, again on the question of whether lower delivery cigarettes carried with them a lower risk of disease to the smoker;

true?
A He did, indeed, review that. He did, indeed, publish that. And his examination was on people who used those products.
Q And his conclusion again was that lower delivery cigarettes carried with them a lower risk of certain diseases, particularly lung cancer; correct?
A That was his conclusion, that's correct.
Q And Sir Richard Dahl, in particular, has spoken to this issue over time; true?
A He has spoken on various occasions with various
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opinions.
Q Okay. And do you know that he spoke to the same issue very recently; that is, in a deposition that was taken this year?
A I have not reviewed that deposition. I can't tell you with certainty what it contains.
Q Do you know what his views are today about whether lower delivery cigarettes reduce risk?
A My understanding of his views today are that there is a dichotomy in the data available from his own study of U.S. physicians and the American Cancer Society studies showing an increase in disease risk or a stable disease risk, and the studies that have looked cross-sectionally at various populations or otherwise at various populations would show a reduction in risk. That there is evidence that he believes suggests a reduction consistent with the introduction of low tar and nicotine cigarettes in England, and that he is at this point in time certain that the reduction is not proportional to the reduction in tar, but believes that there might be a reduction in the risk particularly for lung cancer. That's my understanding of his belief.
(Deposition Exhibit 7 marked.)
BY MR. BERNICK:
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Q I'm going to show you as Exhibit 7 page 181 of the deposition taken of Sir Richard Doll on March 16 of this year. And I've highlighted on this the lines 4 to 15. I want to have you read those for a moment.
A Yes.
Q The testimony that you see from Sir Richard Doll on Exhibit 7, is that a -- is Dr. -- is Sir Richard being reasonable when he makes the statements that he does regarding reduction of risk as you read them on Exhibit 7?
A I think in light of the information that we have as we sit here today, I think that the statements that Dr. Doll has made in his deposition are ones that are not consistent with the data that we have today, and I believe that he might reconsider that on the basis of that information.
Q So you think that Dr. Doll when he testifies, as he does, that there has been a reduction of risk without reasonable doubt, is being unreasonable?
A That is not what I said.
Q That's what I asked you. Do you believe that Dr. Doll is -- that Sir Richard is being unreasonable when he testifies, as he has in Exhibit 7, that there has been a reduction in the risk of disease beyond a

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reasonable doubt?
MR. GRUENLOH: Object to form. Objection; asked and answered.
THE WITNESS: Okay. I believe that Dr. Doll is being reasonable in that he is relying on the reviews that had been conducted. What I said was in light of the evidence we have today, I think he might reconsider that opinion.
BY MR. BERNICK:
Q When you say the evidence that you have today, is this published or unpublished evidence?
A This is evidence that has been presented at the Institute of Medicine and has been discussed in deposition. It has not been published. It will be published in the near future.
Q If we confine ourselves to data that has been published, Dr. Burns, is the statement that Sir Richard makes as you see in Exhibit 7 a reasonable or unreasonable statement?
MR. GRUENLOH: Same objections.
THE WITNESS: If you confine yourself to archaic data, Dr. Doll's statement is consistent with the statements that were made by the Surgeon General reports, by the report you just described by the
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National Cancer Institute, and therefore, are reasonable statements.
BY MR. BERNICK:
Q I didn't ask you about archaic data. I asked you about published date.
A You're asking --
Q Dr. Burns, I'll ask you again.
A -- me to operate off an incomplete set of observations, and then you're asking me to offer an opinion about whether that information is supported.
Q No. A much simpler question. Just focus on the question that I ask you. If we confine ourselves to the data that has been published, that is, we look at the data that is published today, is the statement that appears in Exhibit 7 by Sir Richard regarding reduction of disease from lower tar cigarettes, is that statement reasonable or unreasonable based upon the published data?
MR. GRUENLOH: Same objections.
THE WITNESS: At the time at which Dr. Doll made that statement, it was a reasonable statement based on the published data. That is not to say that it reflects current scientific truth.
BY MR. BERNICK:
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Q Is there -- are there any published research papers, that's gone through peer review, appeared in a peer-reviewed journal, are there any published research papers today which say that reduction of delivery does not carry with it a reduction of risk of disease?
A My understanding is that if you reduce delivery to people, there is compelling data that you reduce the risk of disease. The issue is not whether if you reduce the amount of tar ingested by people there is a disease

reduction. The issue is whether when people switch brands of cigarettes, they actually reduce the yield.

There is a substantial body of data, some of which shows that when you look at cigarette ingestion, that there is incomplete compensation. Others that show that the nicotine yield across the population is very similar across brands with very different nicotine yields. That is, indeed, the body of science that we're examining at this moment. And that is the information we're trying to develop a new understanding of.

Q I'm going to have that question read back one more time, Dr. Doll -- Dr. Burns. And I'm going to see if you can answer it. And if not, I have no more questions this afternoon. We'll just take it up with the judge. I didn't ask for your personal views on what

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the scientific truth is today based upon data that's not published or whatever.

I want -- I asked a question about the state of published science that's available to all of us in published form. I want to know, are there any articles that appear in the published peer review literature which say that lower delivery cigarettes do not, in fact, result in any reduction of risk of disease?

A Yes. There are a series of articles that have been published that examine the rise in adenocarcinoma of the lung and that suggest that the newer low tar and nicotine cigarettes may have resulted in actual increases rather than decreases of the yield. Some of that work has been done by Michael Tune (phonetic). Some of that work has been done by others.

There has also been publication of work comparing the death rates from lung cancer from the American Cancer Society study and from British Physicians study, both of which have been characterized as suggesting that the introduction of low tar and nicotine cigarettes has not been accompanied by a reduction in the risk of disease in those studies.

So yes, there is a body of data that is published that suggests exactly what you asked me to

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opine on, which is that the reduction in risk does not exist.

Q Can you identify a single peer review publication which shows that reduced delivery cigarettes do not, in fact, result in a reduction of the risk of disease?

A I have identified several of those for you --

Q I have --

A -- including the comparisons of the American Cancer Society studies, including the work that Michael Tune and others have done examining the changes in lung cancer risk, and including the comparisons of the first and second phases of the British Physicians studies.

Q Can you give me a citation?

A I can't cite them from anywhere.

Q An author and title or a date?

A Michael Tune. Richard Peto. Recent -- last several years. I can't tell you the specific, whether it's '97, '98 or '99.

Q Anything that wasn't available to Sir Richard

when he testified in March of this year?
A I don't know what was available to Sir Richard when he testified.
MR. BERNICK: Okay. I'm done for this
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afternoon. I reserve my right to continue the deposition when we have the court review the questions that have been put. But I pass the witness to somebody else. Thank you, Dr. Burns, for your time this afternoon and yesterday.
THE WITNESS: Perhaps we could take a break? Is that acceptable to the powers that be?
MR. BERNICK: Off the record.
(Discussion off the record.)
THE VIDEOGRAPHER: Let's go off the record at 3:01 p.m.
(Recess.)
THE VIDEOGRAPHER: This marks the beginning of Videotape Number 4 of Volume 2 in the deposition of Dr. David Burns. We are back on the record at 3:12 p.m.
EXAMINATION
BY MR. STEIN:
Q Good afternoon, Dr. Burns. Nice to see you again.
A Good to see you.
Q I know that you haven't forgotten, but just for the record, my name is Adam Stein, and I represent B.A.T. Industries in this action. I have just a few questions for you, and I will attempt to be brief.
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Now, the last time we met, other than yesterday, was at your deposition in the Blue Cross/Blue Shield case on May 5, 2000. Do you recall that?
A I do recall that.
Q And at that deposition I asked you a series of questions about B.A.T. Industries and whether you intended to offer any opinions at the trial concerning B.A.T. Industries. Do you recall that?
A Yes, I do recall that.
Q And your answer was that it depended on what you were asked. And I gathered from that that at least as far as you were concerned, that you are not intending to offer any separate opinions as to B.A.T. Industries. Am I right about that?
A You are correct.
Q And is that also the case here?
A That is also correct here.
Q Now, you also testified that you were not an expert on corporate governance issues and did not review the expert report of B.A.T. Industries' expert, Professor Robert Stobaugh, which analyzed B.A.T. Industries' corporate structure, and concluded that its relationship with BNW and its other subsidiaries was consistent with generally accepted corporate practice.
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Is that just a fair summary of something you and I discussed at that point in time?
A I believe it's a fair summary of something that I only understand glimmerly.
Q Fair enough.

A Dimly in the mist.
Q Okay. But it is something that we talked about briefly?
A It is something that we talked about briefly, yes.
Q And does that testimony remain the case today? In other words, you haven't reviewed Dr. Stobaugh's report in this case?
A I have not.
Q And you have not undertaken since May to become an expert in corporate governance?
A No, I have not.
Q Since your deposition in May --
A And I can assure you that between now and the time of this trial I will not undertake such an effort.
Q Okay. Since your deposition in May, have you reviewed any documents that relate specifically to B.A.T. Industries as opposed to any other defendant?
A Not that I can specifically recall, no.

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Q In response to a question I asked you in your Blue Cross/Blue Shield deposition about what role, if any, B.A.T. Industries could have had in research undertaken prior to its creation in 1976, you responded in part by stating that -- and this is where the quote begins, "It would strike me as somewhat reprehensible if the legal process allowed a company simply to say, 'We're going to put all our assets in something, call it something else, and pretend we don't have any responsibility.'"
Do you recall making a statement like that?
A Yes.
Q Now, you also stated that you didn't know if that's what happened. And do you recall that?
A That is -- I recall that, and that is still true.
Q Okay. Have you learned anything since last May that causes you to believe that B.A.T. Industries' creation in 1976 as a holding company for BNW and British-American Tobacco Company, was intended to shield assets or to otherwise escape liability in cases like this?
A I have not.
Q And I take it, then, from that answer that you

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have not seen any documents that would support an opinion to that effect?
A I don't believe so, no. None that I can recall.
Q I also asked you at your May deposition whether it was your opinion that B.A.T. Industries should be liable along with the other defendants in that case for the conduct identified in the complaint which you testified about or which is referred to in your expert report. Do you remember that?
A Yes.
Q Now, when I asked you that question the first time around, I didn't include the fact that B.A.T. Industries did not come into existence as the parent of BNW until 1976. And when I asked you if could point to any conduct by B.A.T. Industries that supported your

belief that it was a participant in the conduct complained of, you referred to biologic research undertaken in the period between the 1950s and the mid 1970s. Do you recall that?

A That's correct.

Q Apart from biologic research in the 1950s and the mid 1970s, is there any other conduct that you attribute specifically to B.A.T. Industries that in your

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mind links it up to the conspiracy that is alleged in this case?

A I just need some clarity. We're talking about the holding company, not the --

Q That's correct.

A -- the other?

Q That's correct.

A I don't have any information specific to actions of the holding company independent of the actions of the other companies. However, they are structured, and I am not in a position of understanding the corporate structure enough to -- or the law enough to understand how those responsibilities should be appropriately apportioned.

Q I take it, then, that it remains the case that you can't point to any specific document that indicates that B.A.T. Industries suppressed or caused to be suppressed any research by its subsidiaries in any area, including specifically smoking and health?

A That's correct.

Q Have you seen any documents that indicate that B.A.T. Industries suppressed or caused to be suppressed information concerning the synergistic relationship to cigarette smoke and asbestos?

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A No, I've not.

Q Have you seen any documents that you can identify sitting here today that indicate to you that B.A.T. Industries was involved in alleged attempts to block or otherwise interfere with any proposed ban of smoking in the workplace by Johns Manville or any other entity?

A No.

MR. STEIN: Thank you, Dr. Burns. I have no further questions.

THE WITNESS: You're a gentleman and a scholar.

MR. STEIN: We'll leave that for others to decide.

(Discussion off the record.)

MR. SCHROEDER: Why don't we go off the record?

THE VIDEOGRAPHER: We'll go off the record at 3:18 p.m.

(Recess.)

THE VIDEOGRAPHER: Back on the record at 3:19 p.m.

EXAMINATION (Continued)

BY MR. SCHROEDER:

Q Dr. Burns, you remember my name is Tom Schroeder, and I had given up a balance of my time for

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Mr. Bernick to ask some questions before his plane. So

I would like to continue my examination, if I could?
A That would be fine.
Q All right. Would you agree with me, Dr. Burns, that within a job category for an asbestos-exposed individual, that different people with that same job category can have very different asbestos exposures?
A That is true on an individual level, yes.
Q You testified yesterday that in addressing an impairment on pulmonary function testing that you required, and correct me if I'm wrong, that there be an impairment greater than approximately 20 percent of pulmonary function. I assume as against expected pulmonary function for the individual. Is that fair?
A Actually, what I believe I testified to was that there are statistical algorithms that we use to define with great precision the abnormal values. And we use those to define whether someone is normal or abnormal. On the whole, as a rough rule of thumb, a 20 percent reduction is approximately the level at which someone is abnormal.
Q Okay. And what I wanted to do is follow up with you that if one were to adopt a rule based on your on-the-whole rule, what level of deviation would be

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acceptable from the 20 percent, in your view, that would -- that would either allow for a normal pulmonary function, that is, something slightly more than 20 percent that still would be normal, within your view, or if it goes the other way, something less than 20 percent that would be abnormal, in your view? Do you follow my question?
A I don't follow your question. And let me try and figure it out. The algorithms define where that threshold of abnormality is. If you're talking about whether those algorithms would be slightly above or slightly below 20 percent, I would go and look at the values that we generate with a specific pulmonary function study in our laboratory.
If you're asking me whether the 20 percent value as a number has around it a set of values that are normal, so you have to get to 75 percent below 80 percent, before you consider it abnormal, no, that's not the way it works. A hundred percent is what you expect to derive. We know that there are normal deviances from a hundred percent, normal variation from a hundred percent, but by the time you get approximately 20 percent down, you are in a group where you no longer are included in that group that would be considered normal.

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And that is the threshold for defining somebody as having an abnormal or functional decrement. Abnormal test or functional decrement.
Q If I were to adopt a rule that anything less than 80 percent on forced vital capacity would be deemed abnormal, is it not true that as an arbitrary rule as applied to individuals, that I may find persons on occasion that would be less than the 80 percent who, in fact, would not have abnormal pulmonary function tests as you would compare it to your algorithm?
A That would depend on the pulmonary function measure being used, the methodology being used, and the

construct in which you were doing that, what it is you were trying to accomplish.

Q And if we were using your algorithm that you use in your clinical practice on a day-to-day basis, would it not be true, then, under those circumstances?

A It would depend on the specific measure. If you're looking at something like the forced vital capacity, I believe that 80 percent is outside the normal range. I'd have to go back and look. It would depend somewhat on the age of the individual. If you are using a measure such as FEF 25/75, then a 20 percent rule might not include -- might not exclude everyone who

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is normal.

Q Okay. Is it true as one gets older, their pulmonary function declines naturally?

A The measures of pulmonary function decline naturally, yes. That is accounted for in the algorithms that I described for you earlier. Age is one of the terms that goes into those equations.

Q Would you agree with me that the measurement of abnormality on pulmonary function testing can depend greatly on the group, the comparison group that you use for what's a normal population?

A Well, it -- with different published studies, one has slightly different algorithms in terms of the percentage normal and abnormal that would be expected. To the extent that one is shifting demographic groups, such as using values for adults, for children, or using values for whites, for blacks, one can lead to abnormal characterization.

To the extent that one is using measures for things such as DLCO, then there are some substantial differences in the normal values that have been reported by different groups.

Q And, for example, on that DLCO, would you agree that the differences could exceed five percent on either

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side of the 80 percent?

A I'm not sure specifically what you're referring to. The expected normal values predicted by the equations are substantially different when -- depending on whether one uses the equations from Salt Lake City or from other locations.

Q And that's really what I was trying to get at. Could you give me the range -- when you say substantially different, can you quantify that for me in percentages as a departure from normal?

A I would have to go back and look at that literature to give you a percentage variation. But it is large enough to be clinically significant.

Q All right. Can you tell me, sir, whether it could be more than five percent difference between Salt Lake City on the one hand, and the other studies you mentioned that might be at the other end of the spectrum?

A I can't give you a precise quantitative estimate without going back and reviewing that data. I would not be surprised if it were of that order magnitude or somewhat larger. But I can't give you a precise estimate.

24 Q Do you use the Salt Lake City data as a
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1 comparison group in your practice?
2 A No. I believe we use another set of
3 algorithms, but I don't think it is the Salt Lake City
4 data.
5 Q Would you agree with me that the Salt Lake City
6 data, which is, as I understand it, composed of largely
7 a Mormon population, tends to have a group of people
8 that's used for comparison that have a higher lung
9 function than you might find in other comparison groups?
10 A I don't believe that the Mormon population
11 specifically has a higher level of lung function. The
12 Mormon population living in Utah, particularly around
13 Salt Lake, lives at altitude. And it is the altitude
14 that I believe is the principal concern with using the
15 values for DLCO derived in that population for the rest
16 of the country.
17 Q If one were to use the values of the Salt Lake
18 City population as a comparison group for DLCO, what
19 would that tend to show as compared to, for example, the
20 group you use? Would they tend to make your patients
21 look like they are slightly more impaired? Or
22 substantially more impaired? Or would they make them
23 look like they are less impaired?
24 A It would yield a higher predicted value which
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1 for any given level would generate a higher percentage
2 abnormality.
3 Q Had you considered using the Salt Lake City
4 comparison group for your practice?
5 A I had not specifically considered that. That
6 is a decision made by Dr. Clausen in our group who runs
7 the pulmonary function laboratory. I know that he did,
8 indeed, consider those values and felt that they were
9 not the appropriate ones to be used here in San Diego at
10 sea level.
11 Q Do you know anything about a comparison group
12 used in the state -- comprised, rather, of individuals
13 from the state of Michigan?
14 A You would have to give me a little more
15 information than that. It's been a long time.
16 Q It's developed by Dr. Miller at Mount Sinai?
17 A It's been a long time since I looked at that
18 specific literature, and I don't have a recall of that
19 specific article, no.
20 Q Would it be fair to say that if you wanted to
21 compare a group of asbestos-exposed individuals and
22 determine what their impairment of lung function is,
23 that in order to get an appropriate comparison, that you
24 should try to compare them to a group of other
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1 occupationally-exposed individuals who are, though, not
2 exposed to asbestos?
3 A I'm not sure what your comparison is intended
4 to demonstrate. If your comparison is intended to
5 demonstrate whether they have abnormal function, then
6 the appropriate comparison group would be individuals
7 who have no exposure of any type. No smoking, no
8 occupational exposure to dust or other things, and no

asthma.
If you're interested in whether occupational exposures to dusts and all of the other things in occupational environments are different when asbestos is in the mix, then one would compare an asbestos-exposed population to an asbestos-nonexposed population. To do that, one would have to do something to control for the profound effects of cigarette smoking on most measures of lung function.
Q If you have a group that's exposed to asbestos and they're occupationally exposed, and by that I mean that they are tradespeople, okay, they work in trades that involve asbestos, but they also are around other substances that tradespeople could be around, construction sites, etc., if you wanted to determine the amount of pulmonary function impairment they had that
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you would relate just to their asbestos exposure, and you want to control for the other background exposures, who would you compare them to to give you the best reading you think you could get to, hopefully, isolate the asbestos exposure?
A My recommendation, I think, in that setting would be to compare them to a normal population, to define the magnitude of their reduction, and then to examine the relationships of the various exposures within that population. I think that that would be very difficult in the face of the dominant effect of cigarette smoking on various lung function parameters. One would have to control for that first, and then look for residual effects.
Another way to do that would be to try to examine the known effects of exposures to each of the individual likely exposures, where the prospect of looking for patterns of illness, such as the relatively pure restrictive pattern found with asbestos exposure.
But is impossible, as far as I know, to get a truly matched control group of people who are exposed to asbestos in various occupations, and compared to another group with absolutely identical smoking behaviors and other exposures absent asbestos. That's an unrealistic
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expectation for a control group.
Q Would you agree that the contribution, if any, of smoking to lung function impairment can best be measured by FEV1 over FVC measurement?
A The question you're asking is expressed globally. And if you express it globally, no, that is not the best measure. The best measures are in order of age at which they first appear. You can see changes in the small airways with closing volume. You can see changes in the small airways that progress to more permanent changes, that is, ones consistent with chronic obstructive lung disease, first in the FEF 25/75. You then see those measures translate into a reduction in the FEV1.
The question I think you were driving towards is, in a combined exposure where you have both restriction present due to asbestos and obstruction due to the effects of cigarette smoking, in that setting, then the measure that gives the best measure of

obstruction is the FEV1 over FVC, and the best measure of restriction is the total lung capacity.

Q When one has a pulmonary function analysis done, typically -- that is in, I suppose in an office setting like yours, or in a laboratory, in a

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pulmonologist's office, typically in the battery of tests that are run is FVC one of the standard tests that's conducted?

A Yes.

Q Is FEV1 over FVC a standard test?

A Yes.

Q Is TLC a standard test?

A TLC is part of a more extended test, ideally done with a body plethysmograph. It is a relatively standard test in our institution. It varies in other institutions across the country, and is nowhere near as widely available nor as easy to determine as is the simple spirometric measurements of forced vital capacity and FEV1.

Q And is DLCO something that's typically done as a standard test in your office?

A It may be done as a standard test, or it may not, depending on the individual laboratory. I believe our lab does it as part of their routine spirometry, but I'm not a hundred percent certain.

Q And the FEV 25/75, would that also be one of the standard tests conducted?

A That would be -- that's not a separate test. The test is a forced expiration. And the FEV1 and other

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measurements are standard measurements made from that test. And so yes, most laboratories would calculate that value. The FEF 25/75 and MEF 25/75 mean the same thing. They're just different terminologies for the same measure.

Q If, then, I were to have an exam done of me on spirometry, and done in your office, then the standard tests would include the FEV1, FVC and the FEV 25/75? Did I say that right?

A Yes. It would include those, plus a series of other calculations.

Q All right.

A Now, I just, for the record want to be clear. You wouldn't have that done in my office. You would have it done in the pulmonary function laboratory at the hospital. That's how we do them in our practice setting. My office would simply produce a pile of paper that might fall on you, is all.

Q And then if I had those tests done, would you then be prepared -- and if I were a smoker and exposed to asbestos, would you then be prepared to tell me what you thought the contribution of smoking was to my impairment and the contribution of asbestos to my impairment?

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A If I was seeing you if my office, I would do a history, a physical. I would explore your occupational history. I would review your medical records. I would review your chest X ray and a variety of other

laboratory data, as well as your pulmonary function studies. And then I would be in a position to tell you what my best judgment was about the relative contributions of various types of processes to your current clinical setting, and how those were reflected in your pulmonary function.

Q Could you tell me that based on the pulmonary function results alone? The impairment issue.

A I'm not sure what you're asking.

Q Could you tell me if I were a patient of yours and I had my pulmonary function tests run at your laboratory, and then came in and saw you as a patient, could you tell me what proportion of my impairment you believe was caused by my cigarette smoking, and what proportion of my impairment would be caused by my asbestos exposure?

A Number one, that wouldn't be what I would do in practice, and we've discussed that. Number two, that would depend on the nature of the abnormalities present on your pulmonary function. If your pulmonary function

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was absolutely normal, I would obviously be able to tell you that there was no evidence of pulmonary function abnormality.

Q Sure.

A If you had a profound obstructive defect, then I could tell you that the principal consequence that you had suffered was likely due to cigarette smoking. If you had a reduction in a forced vital capacity with preservation of your expiratory flow rates, then I would tell you that your principal effect was one of asbestos exposure. If you had a combination of those, with very small lungs, and a marked degree of expiration, I would tell you that you had a substantive contribution from both.

Q And could you then tell me based on the pulmonary function results how you would allocate that substantive contribution, percentage-wise, to asbestos versus tobacco based on how you read my pulmonary function results?

A You would allocate that based on the judgment about the extent of the disease that was present that was restrictive in comparison to the extent of the disease that was obstructive. There are, of course, individual cases where that allocation would be more

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difficult.

Q Okay. By and large, is it fair to say that you could make that allocation in most people based on the pulmonary function results?

A That would depend on what the results were. In the vast majority of individuals that you examine, there is not severe mixed disease. Okay? And those individuals with severe mixed disease, it can be quite challenging to allocate the disease to restriction and obstruction. In those instances where there is a predominant disease, it is much easier.

Q Okay. In the 1985 Surgeon General's report you wrote -- and I say you, because you were the -- either author or had the editorial control over it. You wrote that, "It's clear that if cigarette smoking contributes

to the development of interstitial fibrosis in asbestos-exposed workers, the contribution is a minor one in comparison with effect of asbestos dust exposure." Do you remember that?

A Yes. That's correct.

Q Do you agree with that?

A I agree with that that the mechanistic basis for the development of the interstitial fibrotic pattern that we characterize as consistent with asbestosis is

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predominantly the result of exposure to asbestos. That there are minor contributions to fibrosis produced by cigarette smoking. I have modified that since that time based on information that has become available that there does appear to be evidence that people who smoke are likely to have a higher asbestos burden in their lung, perhaps from interference with clearance mechanisms. And, therefore, they might have greater asbestos exposure-causing fibrosis. I believe we --

Q I think we covered that last time.

A -- covered that.

Q And is it fair to say based on that that it was your opinion that while there's still more work ongoing in that area, there lacks scientific consensus on the clearance mechanism as being, in fact, the mechanistic factor for the burden? Is that fair?

A That is fair. And there is also some ongoing discussion about whether the burden is actually increased. I mean, my reading of that evidence at the moment is that the preponderance of the evidence suggests that it is increased. But there is ongoing discussion of that as well.

Q And, in fact, what -- if the burden is not, in fact, increasing, what's the other apparent result?

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A I'm not sure what you're saying.

Q You said there's evidence that the burden may, in fact, be increasing. That evidence is based on X ray readings in prevalent studies?

A No. Those are based on, actually, as I recall, lung digestion studies. And I believe there's also some animal experimental data to suggest that for a given controlled amount of asbestos inhalation, that there's greater retention in animals exposed to cigarette smoke. That's one potential explanation for the unequivocally supported observation that asbestos workers who smoke have higher abnormalities on chest X rays than asbestos workers who don't smoke.

The other potential explanations are that with cigarette smoking there is an influx of inflammatory cells into the lungs. Those inflammatory cells may facilitate or accelerate or accentuate the response to asbestos in the lung that results in fibrosis. There are probably other explanations that have been put forward as well.

Q And it remains also possible, though, does it not, that, in fact, there is no increase associated with smoking? Isn't that one of the possible explanations as well?

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A Increased what?
Q Increased level of fibrosis that's caused by cigarette smoking.
A You've mixed two pieces there. Okay? I don't think that there is substantive debate that cigarette smokers who are exposed to asbestos as a group have more severe X ray abnormalities than cigarette -- than nonsmokers who are exposed to asbestos in the same occupations. I think that's a relatively well-accepted fact.

The question is, why is that happening? There are several possibilities why that may be happening. It may be happening because of combined disease processes, inflammation and fibrosis crossing thresholds earlier. It may be because there is a greater retention of asbestos fibers in the lung, and therefore, a greater exposure to asbestos.

It could be because of damage due to smoking interfering with the clearance of the lung. Or it could be due to the inflammatory response produced by smoking in the lung facilitating the fibrotic response produced by asbestos in the lung. And there may be other explanations as well.

Q And one of the other explanations might include

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the fact that if more fibrosis is seen among smoking asbestos workers, it could be not a causal relationship, but related more to the fact that asbestos workers who smoked tended to be the asbestos workers who also had more asbestos exposure; is that a fair interpretation of a possible result?

A I think that was one of the issues that was of concern in the epidemiologic literature that examined those questions. I think, it is my belief, that that issue has largely been put to rest. That there is, indeed, a real, i.e., not explainable by differences in occupational exposure, difference between smokers and nonsmokers.

Q Can you point me to what literature you would rely on for that proposition?

A There's a variety of literature that has examined that. One of the pieces that I think synthesizes that reasonably well is Dr. Nicholson's report in this litigation.

Q Have you read Dr. Nicholson's report?

A I have.

Q You're aware of the fact, are you not, that the studies he relies on principally are smoking-prevalent studies that look at smoking versus nonsmoking among

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asbestos workers for potential parenchymal abnormality; right?

A I think your question is not phrased correctly.

I don't believe that they were smoking-prevalent studies. I believe that they were studies of the prevalence of chest X ray abnormality in populations.

Q That's what I meant. And you would agree that that's what he principally relies upon?

A Most of those studies were cross-sectional studies that define the prevalence at a point in time of

abnormality, yes, that's correct.
Q Would you agree with me, Dr. Burns, that when you look at the more recent multivariable regression studies that look at the same question of whether smoking increases the level of parenchymal abnormality among asbestos workers, that the multivariate studies show that in some cases there is no smoking effect, and that in other cases, if there is a smoking effect, it cannot be distinguished statistically from background? Would you agree with that?
A It would not surprise me given the variability in the data that it is very difficult to model that data. I would have to go back and look at specific

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articles to respond appropriately to your question. Even in the cross-sectional studies not 100 percent of those studies showed a difference between smokers and nonsmokers.
But I do believe that the preponderance of the evidence at this point in time, since you asked, is that smokers have a higher frequency of abnormal chest X rays by the ILO classification. That is true for smokers as a group compared to nonsmokers, absent asbestos in both groups. And I believe that that is the consensus view of the evidence for asbestos-exposed populations. The difference, I think, is in one of how big that difference is and how broadly applicable across all of the ILO classifications schema it is.
Q Do you intend to offer an opinion in this case as to what that difference would be as applied to the Manville Trust claimants?
A I have done no analysis of the Manville Trust claimants and do not expect to offer any information or testimony specific to the Manville Trust claimants other than those things that are present in the general medical literature.
Q When you reviewed Dr. Nicholson's report you saw, did you not, that as to pleural plaques he reported

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his opinion that smoking actually causes an increase in pleural plaques among asbestos-exposed individuals; right?
A Yes, I did.
Q And did you notice as well that his conclusion did not reach a statistically significant result as based on a 95 percent confidence interval? Do you remember that?
A My recollection is that he reviewed that, and when he included all pleural change, it was significant. But when he limited it to pleural plaques, he did not find a significance. But I would have to go back and look at my piece with more recency if you want me to opine on it.
Q All right. And is it fair to say that to the extent he finds an increase -- strike that.
To the extent that Dr. Nicholson concludes that there is an increase of pleural plaques due to smoking, that that's his opinion, that to the extent he does say that in his report, that you would disagree with that?
A It is my view of that evidence, okay, that it does not support a causal relationship to smoking.

Q Thank you. And as to Dr. Nicholson's analysis of the apportionment of excess risk in lung cancer
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between asbestos and smoking, based on your testimony you gave, I think this morning, and probably some yesterday, is it fair to say that you essentially agree with his analysis as he sets it forth in his report?
A I don't disagree with his analysis. I think that that is a valid approach to this problem. My preference is to do that with somewhat more specificity on the actual population. But I think that it is certainly a legitimate approach that he is taking.
Q Okay. And, in fact, in your 1982 book, which was Exhibit, I believe, Number 4, on page 142 -- and I think there's a copy in front of you there, Dr. Burns.
A Okay.
Q Could you take a look at that?
A The "Asbestos, Smoking and Disease"?
Q Yes, Doctor. On page 142.
Let's start with page 141, if we can.
A Okay.
Q Do you see page 141 there's an introduction to the section on lung cancer? I believe that's what that is.
A No, it's actually an introduction to --
Q The interaction section.
A The whole interaction section, yes.
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Q That's fair. This sets forth the summary of some of the conclusions that are to follow in the chapter; correct?
A Well, it sort of lays out how we're going to approach it, yes.
Q All right. And you say there in the second paragraph that --
A Second paragraph, beginning with, "In general"?
Q Hold on just a minute. I think that's where I want to go.
A Okay.
Q Well, actually, in the first paragraph, the third sentence, do you see where it says, "It is clear that both agents," referring to asbestos and smoking, "produce a broad range of responses in exposed individuals"?
A Yes.
Q And, "Therefore, it is not surprising that these agents have a number of complex interactions in the health of individuals with combined exposures." Do you agree with that today?
A I agree with that today.
Q Then in the next paragraph you say, "In general, cigarette smoking and asbestos exposure might
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interact in a number of ways." And you say it might in the following ways; right?
A That's the word that was used, yes.
Q All right, fine. And you say, "Both agents might produce the same disease. For example, lung cancer. The combined exposure might result in a number of cases equal to the sum of the two agents acting

separately, an additive effect." Do you see that?
A I see that. I'm unsure as to what you're asking me to do other than validate the fact that those words are present. Is that all you're asking me to do?
Q No. At that point, yes. Now I'm going to ask you, do you agree with that today?
A I agree --
Q In some cases that that exposed -- the combined result might be an additive effect?
A I think you're misunderstanding this paragraph, or this set of issues.
Q Well --
A This set of issues is simply identifying theoretically the various potential forms of interaction that might exist. It is not an effort to define the nature of those interactions. It is simply laying out the various forms of interaction that might potentially

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occur. The subsequent chapter actually deals with the data and whether those interactions do occur.
Q Would you agree with me that there can be circumstances on low asbestos exposure and low tobacco exposure where you might have the two agents acting separately and producing an additive effect? Is that a possibility?
A Are you talking about for lung cancer?
Q Yes.
A Okay. My understanding of that literature, and my understanding of both the epidemiology and the biology, is that, no, that is not true; that what happens is if you have very low dose exposures, you have very low risks that are multiplied. When you multiply a risk, say, of 10 for smoking by a risk of point 1, okay, for asbestos exposure, because there's a very low dose of asbestos exposure, then one would have a tiny adjustment in the number 10, and that that would not be easily differentiable.
That multiplying 10 by point 1 would give you a 11. That might not be easily differentiated from 10.1, which would be the additive effect. And so in some studies you might have the appearance of addition. But my understanding of the way this is viewed

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scientifically is that that is a multiplicative interaction throughout the entire range of the dose response relationship. It's just as you get to very low doses, the difference between multiplying something and adding it becomes very small in absolute terms.
Q Can you cite to me, Dr. Burns, what literature, what studies you rely on that establish that at low dose exposures the relationship is multiplicative and not less than multiplicative or, indeed, perhaps additive?
A The basis of that opinion is the general biologic literature on the effect of carcinogens on the occurrence of lung cancer, or on the occurrence of cancer. And that is a multistep process occurring sequentially with large numbers of steps over a period of time.
That what we understand about that process is that there is no threshold below which an effect disappears, but that the effect is proportional to the

dose and potency of the carcinogen that it appears over time. Okay?

That is then combined with the evidence that exists on asbestos exposure in various different settings, okay, where the magnitude of the asbestos exposure has been shown to influence the magnitude of

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the risk. And so there is a clear dose response relationship that almost everyone feels exists for asbestos exposure and the occurrence of lung cancer.

There is also a very clear dose response relationship with no threshold for cigarette smoke exposure and the occurrence of lung cancer. In the studies that have examined those issues, okay, of synergy where you have combined data, it appears that the multiplicative interaction which was demonstrated at high doses of both in the Selikoff data appear to persist as you go down to lower doses of asbestos exposure and lower relative risk of lung cancer.

And that, therefore, when you put all of that together, you would -- you are left with a statement or a conclusion that this -- the evidence exists that the dose response relationship exists throughout the entire range of doses; that the multiplicative interaction appears to exists for all ranges of doses where it can reasonably be expected to be demonstrable. And therefore, it is reasonable to expect that it would persist below those levels at which it is possible to measure because the magnitude of the effect is so small that one cannot see it.

MR. SCHROEDER: Could you mark that for me?

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Q Can you cite to me a study for that proposition that involved -- let me preface this. There are, I believe, approximately 12 -- approximately 12 studies that provide data that are sufficient from which one can draw quantitative conclusions as to whether there's interaction that deal with the issue of lung cancer in asbestos and smoking.

Can you cite to me a study that supports the proposition that at low levels of asbestos in smoking exposure there is, in fact, a multiplicative effect?

MR. GRUENLOH: Objection; form, foundation.

THE WITNESS: You're asking for a study on something that cannot be demonstrated. I believe that there have been reviews of the relationships across different studies that have suggested that for the ranges where an effect is demonstrable and measurable, that there appears to be a multiplicative action.

What we understand about the normal course of examining lung cancer, or any cancer, is that you would back-extrapolate from those dose response curves. That is a relatively standard practice that is used routinely in examining occupational carcinogens and other exposures.

And so I can't point you to a specific study

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that looks at something that is not measurable and not definable, because that study is not possible to do.

What I can point you to is examinations of that

body of literature that suggest that the multiplicative interaction is operative over the full range of dose response relationships that have been established for asbestos where that has been examined in any way that you could measure it.

And then I'm simply making the assumption that is widely used in looking at low dose exposures, which is to extrapolate downward from higher dose exposures where you have good data because of larger effects to impute what is likely to happen with lower dose exposures.

BY MR. SCHROEDER:

Q What authority do you rely on that you would cite me to for the latter proposition? And that is, people who, I think you said, have drawn that conclusion based on other literature that you can look at low dose exposures and still reach the same result?

A There is a substantial body of literature around risk assessment measures for environmental factors for exposures both for respiratory disease and also exposures particularly for occupational carcinogens

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where the data routinely is taken from higher dose exposures where there is human data and extrapolated downward to establish thresholds for acceptable occupational levels for those exposures.

That is routinely done by the Environmental Protection Agency, by various other groups that set threshold limits. And I believe that there are review and -- I don't know whether they would be guideline documents or other documents, that the FDA and other groups have prepared on risk assessment that support that approach.

Q Can you point me to a document that deals with that issue in the context of asbestos and smoking? And if so, I would like to know what name -- either the name of the document or the author if you could, please.

A I believe that there is an examination of that issue -- I think it has been examined by several individuals. There have been several reports that I'm familiar with. There is at least one published, I believe by Julian Peto, but I could be wrong, some 15 or 20 years ago now that looks at the data for lung cancer and defining a dose response relationship with fiber years of exposure and extrapolating that down to a relative risk of one. That is, no background risk.

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I believe that others have done similar type of extrapolations from the data. I believe that either Newhouse or Berry have done that type of calculation. I believe as well that McDonald has done that type of calculation based on the mining data.

There has also been that type of assumption that provided the core substrate for examination of very low dose asbestos exposure in the general environment, and that was a report that was published some 15 years ago or more that defined the -- maybe less than 15 years ago. I think it was shortly after the 1985 Surgeon General's report. That defined the impact of or expected impact of nonoccupational exposure to asbestos; that is, schools and buildings, etc. And so there's a

wealth of documents out there that have done that in relation with asbestos.

Q In connection with smoking? I think I limited my question.

A Yes, in connection with smoking.

The data from the British studies, Newhouse and Barry, control for smoking. And I believe that some of the McDonald data control for smoking, although I'm not certain on that one. I do know that the estimates are generated differentially for smokers and nonsmokers.

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And the report on ambient asbestos exposure, that does that downward extrapolation.

Q Is the Barry study you were referring to the same one we talked about either earlier today or yesterday afternoon? That is, Barry 1972?

A No. It is that subsequent body of work that has been published.

Perhaps we could take a short biologic break here.

MR. SCHROEDER: Okay.

THE VIDEOGRAPHER: Off the record at 4:04 p.m.

(Recess.)

THE VIDEOGRAPHER: We are back on the record at 4:12 p.m.

BY MR. SCHROEDER:

Q Dr. Burns, let's go back to page 141, if we can, of your book.

A Okay.

Q And you say there, do you not, in the paragraph numbered number 1, that, "Both agents," that is, smoking and asbestos, "might --" "combined exposure from both, rather, might result in a number of cases equal to the sum of the two agents acting separately an additive effect"; isn't that correct?

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A That is the correct reading of those words. As we've discussed previously, that is the initial step in laying out the potential or hypothetical ways, in contrast to actual ways, that smoking and asbestos might interact.

Q But you don't say here, though, do you, Doctor, that, in fact, this is only a hypothetical; that is not, in fact, what could be occurring?

A I believe that a reasonable reading of that introductory paragraph in the first sentence would lead the reader to believe that what we were doing was laying out the potential ways that asbestos and smoking might interact as a basis for them reviewing the data to describe how it actually interacts.

Q Okay.

A That was certainly my intent in writing it. And if I did not effectively communicate that, then that is a limitation in my communication skills.

Q On page 142 in the first paragraph under "Lung cancer," in the second sentence you say, do you not, "In all of the groups studied," and that is studied for interaction, "those workers with a combined asbestos and smoking exposure had more lung cancer deaths than would have been expected from the sum of the independent

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effects of cigarette smoking and asbestos exposure, indicating that there is a synergistic effect of the combined exposures on lung cancer death rates"; right?
A That's correct.
Q And do you agree with that today?
A I agree with that.
Q The next sentence says, does it not, that, "The magnitude of the synergism varies somewhat in the different populations studied as would be expected from the differing exposures to asbestos and different smoking behaviors"; is that correct?
A That's correct.
Q And do you agree with that statement today?
A Yes.
Q And so what you are saying is that depending on the exposures to asbestos and smoking, the level of synergism, as you define it, that is, a departure from additive model, depends on what those independent exposures are. Is that a fair statement?
A I think that's a potentially mischaracterized statement of what I've said. What I'm saying there is that there are different doses of exposure to asbestos in different studies. There are different doses of exposure to smoke in different studies. And therefore,
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the combined risk comes out differently.
Q Well, the magnitude of the synergism, as you state here, refers to the level of synergism, that is, whether it is additive or all the way to the level of multiplicative, does it not?
A No. Magnitude is, in general, a quantitative effort. The synergism is the combined effect above that to be expected from the independent exposures. That is a function of magnitude of the independent exposures. If you have a higher level of asbestos exposure, you will have a higher magnitude of the synergistic effect. If you a higher level of smoking, you will have a higher magnitude of the synergistic effect.
If your relative risk, for example, from asbestos exposure is 2, and your relative risk from smoking is 20, your multiplicative interaction would be -- I'm sorry, relative risk from smoking is 10, your relative -- your multiplicative effect would be one of 20.
If your relative risk from asbestos is 5, and your relative risk from smoking is 10, your multiplicative interaction would be 50. The magnitude of that synergistic effect would be dramatically increased.
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Q You remember the 1980 study that Dr. Selikoff did of the Amosite factory workers?
A Which one? The one in short-dose exposure? Is that the one you're referring to?
Q The one of approximately 500 factory workers where he found an approximately 80 times risk based on a comparison to totally unexposed individuals.
A I'm generally aware of that. It's been a while since I've looked at that particular study.
Q You recall, do you not, that in that study

Dr. Selikoff concluded that the relative risk for asbestos exposure alone for lung cancer was approximately 25?

A I know that that number has been in the literature. I know that it is based on small numbers. I know that when we examined that study for the Surgeon General's report, we didn't feel that the data were sufficiently precise in their estimation to include it. But -- include that as an estimate, 25.

Q How many deaths among nonsmoking asbestos workers do you recall were reported in that study?

A As I said, it has been a while since I reviewed that study. If we need to talk about it in that level of detail, I would need to look at the actual paper.

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Q How many deaths among nonsmoking asbestos workers were reported in the 1979 Hammond study?

A Approximately 450.

Q I'm sorry, how many deaths among nonsmoking asbestos workers were reported for lung cancer in the 1979 Hammond study?

A Depending on the criteria you use, either four or five.

Q Okay. And when using death certificate, it was four, was it not?

A I believe so.

Q Do you recall whether the number of deaths in Hammond -- in Dr. Selikoff's 1980 study for nonsmoking asbestos workers was actually larger than the number of deaths in that '79 study?

A I'm sorry?

Q Do you recall whether the number of deaths for lung cancer among nonsmoking asbestos workers that were in Dr. Selikoff's 1980 study was actually larger than the number of deaths in the '79 study for the same group?

A As I said, it has been a while since I have reviewed the Selikoff 1980 study. It would take me some time to review that. In order to talk about the details

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of the number of deaths, etc., it would require that I go back and look at that study again.

Q Would you agree with me, Dr. Burns, that the precision of statistical estimates increases when you have more deaths to study in epidemiological studies?

A Well, all other factors being equal, a statement that is never true in epidemiologic studies, the number of deaths increases the precision or the reliability of the estimate. That is dependent, then, on other factors, such as the quality of the data that you're using and the population you're examining.

Q On page 142 of your book in the next to last paragraph you say, last sentence, "However, the numerical values of the multipliers may vary for both asbestos and smoking with the time interval since the beginning of exposure and on the intensity of the exposure." Do you agree with that statement today?

A I do.

Q If one were to estimate the relative contributions of asbestos in smoking for lung cancers reported in a population of individuals, you would need,

then, to look, according to your statement here, to the time interval since beginning exposure for each person, the intensity of their exposure; is that correct?

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A No.

Q Why not?

A If one is interested in making an estimation of occurrence of disease or proration of disease or other characteristics in the population, one can use the information that is available. The precision or ability to define with small increments differences is enhanced by having more observations and by having more accurate information on the dose intensity and duration of the exposures in question.

If one does not have that, one drops back and uses larger categories of data, as is commonly done in occupational studies, such as occupation, or occupational category, or industry group, or smoker, nonsmoker, without categorizing the dose and duration of smoking status.

So any one of those approaches can be used to answer those questions. The more data you have, the more precise your estimates can be. But all of those approaches can and have been used.

Q Do you know anything about the manner in which the data work were collected, if at all for the Manville Trust claimants on their actual exposures to asbestos?

A I'm not sure what you're asking. I'm generally

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aware that that information was collected as part of the litigation. I'm generally aware that it was extracted from a combination of sources that vary across time, including such sources as medical records, deposition testimony, affidavits or other filings by attorneys, and presentation of information by attorneys. So there's a variety of sources. I'm not specifically conversant with that data set or the methods by which that data have been retained or tested in any way.

Q You don't know what percentage of the claims files contained information about exposures apart from that reported by the claimant and/or his or her attorney on the claim form, do you?

A As I've now said, I think four or five times, I have not conducted any analysis or any examination on the individual claimants for the Manville Trust.

Q Okay.

A And that includes examining where the sources of information came from.

Q Okay. Would you agree with me, Dr. Burns, that the manner in which one collects information for an epidemiological study can affect substantially the validity of any of the results that you want to draw from the study? That is, that you can have bias in the

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collection and reporting process.

A The more accurate the data is, and the more free from bias the data is, the more valid and more likely it will be to demonstrate a result. It is uncommon for inaccuracies in data to generate a result that is not one that is true. In general, inaccuracies

in data interfere with the ability to demonstrate a result rather than demonstrating a spurious result.

As to bias, bias may influence the results in a random way, or may influence the results in a systematic way. To the extent that it influences results in a systematic way, it has the potential to distort the effect that you're looking at, depending on the power of the bias and power of the underlying effect that you're looking for.

Q Would you agree with me, Dr. Burns, then, in the context of persons making claims for compensation and reporting on a self-reporting basis the level of asbestos exposure they may have had, however described, whether by job description or by industry, etc., that there is inherent in that process the likelihood of bias?

A I think that that's an overly broad categorization. Okay? Certainly it is less likely that

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people are going to be biased in their descriptions of where they worked, as that information may be readily accessible through a variety of sources. If you're asking people to categorize their individual exposures, some people may be biased in litigation, other people will not. It is an issue that would need to be considered. It is not necessarily one that would interfere with the calculation of effects. It's potential, but not necessarily real.

Q Are you aware of any instance in which, in the history of your involvement with Public Health Service and the various public health groups, that anyone in those instances has prepared for public health purposes epidemiological studies based on information filed by claimants for compensation? And if so, please tell me what they are.

A I can't cite from memory that kind of literature. I would be surprised if some of the data sets that have been collected for purposes of litigation have not resulted in some academic publications. But I can't cite them specifically for you.

Q Okay.

MR. MOLSTER: I move to strike everything after the first sentence.

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THE WITNESS: Including the fact that I can't cite them for you?

BY MR. SCHROEDER:

Q Let's move to page 145 of your book that you did for Commercial Union.

A It's a bad sign that we're not getting ground faster on this report. Go ahead.

Q You say in the first full paragraph there, Dr. Burns, that, "An understanding of these dose response interactions, and of the changes over the years in the exposure dose for cigarettes and asbestos, is particularly important for projecting the public health impact of this interaction on this population."

A That's correct.

Q Do you agree with that statement today?

A That's correct.

Q Would you agree with me that the individuals

who were exposed to asbestos in the Selikoff studies had a heavier exposure to asbestos than individuals who were asbestos insulators in the time period following the period of study in the Selikoff studies?

A I'm sort of at a loss as to what your question is.

Q The people -- let me rephrase it, then.

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A The people in the Selikoff study were people who were employed who had had 20 years or more since onset of exposure in that particular union. Okay? That's a defined group.

Q Let me withdraw -- I'm going to withdraw the question and rephrase it so we can get back on track.

A I need some definition as to the group you want me to compare them to.

Q Would you agree that the people in the Selikoff studies are more heavily exposed to asbestos than persons who would have been exposed to asbestos in similar occupations in periods after the Selikoff study was conducted?

A Overall, as a large generalization, that is probably correct. On the other hand, there are obviously individuals and locations where the exposure may have been very heavy.

Q But on average, the people who form the cohort of the Selikoff studies were, by and large, much more heavily exposed than we tend to find in people who would have been studied in periods after the people who were studied in the Selikoff group; correct?

A That's a different question than you had asked previously. You had asked previously about insulators

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in other locations besides the Selikoff study. I think that it is fair to say that the Selikoff population was very heavily exposed. In general, okay, with obvious likely individual and group exceptions, they were more heavily exposed than the insulators in other occupations all across the United States.

It is also true that the extent of asbestos exposure has diminished over time as standards for environmental asbestos limitations have both been reduced and been more aggressively and widely instituted. So there over time has been a decline in the magnitude or intensity of asbestos exposure at any given work year.

Q When you say in this sentence we just referred to that, "One needs to take into account the changes over the years and the exposure dose for cigarettes," what specifically are you referring to?

A There has been a decline in both initiation of cigarette smoking, and also in the prevalence of cigarette smoking over time. That change has occurred nonuniformly across the population, both in terms of the occupational exposures or occupational groups in terms of the smoking behavior, and also in terms of age and gender differences across the population, as well as

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racial differences across the population.

And so one of the issues, if -- that if one has

the data, one should consider in examining future public health effects is the impact that changes in smoking behavior going forward in time are likely to have, or changes in recent smoking behavior are likely to have on future occurrence of disease.

Q Wouldn't that come into account anyway in applying under the way you look at it the level of asbestos exposure in any population with the level of smoking exposure, given the fact that you say it's multiplicative? In other words, why do you need to take this into account when, in fact, when you look at any given population you would automatically take that into account because you would look at whatever the smoking prevalence was?

A If you are looking at the smoking prevalence, then you are taking that into account.

Q Okay.

A You can take it into account with a variety of different levels of precision, however.

Q You say in the last sentence that starts that page, "The asbestos multiplier," and you put it in quotes, "either for groups of current workers who have

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been exposed only to these lower levels or for groups of older workers who have had low exposure levels in the past, should be lower than the multiplier derived for older workers with high exposures." Do you see that?

A Yes.

Q Do you follow? The multiplier you're talking about is the -- the level of interaction between asbestos and smoking, is it not?

A No. It is the asbestos multiplier. In a multiplicative interaction there are two multipliers. One is asbestos and the other is smoking in the context of multiplicative interaction between smoking and asbestos exposure. This chapter -- or this paragraph refers to the multiplier for asbestos. In the Selikoff data, it would be -- the multiplier for asbestos is 5.

Q You say on page 146 in the bottom paragraph that, "No one risk multiplier describes all smoking habits or all asbestos exposures." Do you agree with that statement today?

A Yes.

Q All right. On page 148, then, you talk about cessation of both -- the effect of cessation on smoking and the effect of cessation on asbestos exposure. You say that, "The relative risk due to asbestos exposure

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should stop increasing upon cessation." Is that a fair statement today?

A I believe that that's a fair statement today, yes.

Q Then you further say, though, "There's no evidence that this risk decreases with the time since the last exposure." Is that a fair statement today?

A I believe that's a fair statement today.

Q However, the risk related to smoking, in fact, decreases, I think you've testified many times before, upon cessation of smoking; correct?

A Relative to the risk of the continuing smoker.

MR. SCHROEDER: Okay. Let's mark this one.

(Deposition Exhibit 8 marked.)
BY MR. SCHROEDER:
Q Dr. Burns, I'm now going to hand you what's now been marked as Exhibit Number 8.
A Yes.
Q Do you recognize that as an exhibit that you used in your testimony in the Cimino litigation in Texas?
A I believe it is, yes.
Q And you testified in the Cimino litigation on behalf of whom?
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A I believe it was on behalf of Pittsburgh Corning.
Q And this chart that's shown as Exhibit Number 8 is meant to show the relative risks of developing lung cancer for occupations -- certain occupations of asbestos exposure as related to cigarette smoking, both divided into current smokers and former smokers. Is that a fair statement?
A It is. It actually reflects individual studies in those occupations, yes.
Q Does this chart reflect a fair representation of those studies that address the various asbestos exposures and smoking exposures as depicted on Exhibit 8?
A I think it's a reasonable description of the smoking exposures. It's not a complete description of the asbestos exposures. It is a relatively complete description of the high level asbestos exposures with high relative risks. It leaves out many of the studies that have been done in other occupations where the risks are much lower, for example, chemical workers, or shipyard workers, railroads, those kinds of things. There is one study of shipyards.
Q For purposes of the asbestos exposures as
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depicted on your Exhibit 8, these are fair representations of the studies in those asbestos groups, are they not?
A They are representations of individual studies.
Q I understand.
A If one looks at all of the studies that have been done, okay, I think one will see that the risks are substantially lower on average when one looks at all of the studies. This was intended to show those studies with very high risks.
Q And the high risks would be based on persons who had heavier asbestos exposure; correct?
A In general that's correct.
Q And in the Cimino case, the claimants in Cimino tended to be more heavily exposed, did they not?
A I don't have evidence to describe their average exposure. My expectation from having seen a sample of those individuals was that they would be heavily exposed, yes.
(Deposition Exhibit 9 marked.)
BY MR. SCHROEDER:
Q If you take a look at Exhibit Number 9, can you confirm for us that that's another exhibit you used in your Cimino testimony that shows the years of asbestos

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exposure based on duration for persons in the Cimino lawsuit?
A It was not part of my testimony. I don't believe that it was allowed to be introduced in the court. But it was certainly something we prepared from some of the data provided on that population.
Q Does Exhibit Number 9 accurately reflect as prepared by you the duration of asbestos exposure for the claimants you were dealing with in the Cimino case?
A It reflects the graphing of the data that we were provided on duration of exposure for that group.
Q And you prepared Exhibit 9, or had it prepared at your direction; correct?
A I prepared the graph. I did not collect the data.
Q So the data was represented to you by the claimants or somebody on behalf of the claimants, and you then prepared a graph of it. Would that be fair?
A My understanding is that the data was collected as part of an examination by questionnaire of the claimants conducted by the defense in the case. But I'm not a hundred percent certain of that.
Q Okay. Looking at Exhibit 9, then, is it fair to say that Exhibit 9 shows that most -- the bulk, if
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you will, of the claimants that you were dealing with in the Cimino case had 20 or more years of asbestos exposure. Is that a fair statement?
A Yes, that's a fair statement.
Q And that, in fact, many of them had between 30 and 40 years of asbestos exposure; is that a fair statement?
A That's a fair statement.
Q Then if you go back to Exhibit 8, is it fair to say that the studies that you selected for inclusion in Exhibit 8 reflected the type of exposures that you would have found in Exhibit 9, so that it applied to your testimony in the Cimino case?
A I think that that's a complex question. Okay? Obviously there were very few people in that case that had mining exposure. There were a few, I believe, that had some friction exposure. Very few that had manufacturing exposure. A substantial number that had exposure to asbestos cement, but not in the same setting that the asbestos cement studies were conducted, in that they weren't in the manufacturing plants, per se.
There were a number of people who were, indeed, insulators, spraying insulation. There were a fair number of people who had exposures in shipyards and in
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work conditions that might be comparable to shipyards. That is, general maintenance and repair of facilities where asbestos was in the air in substantial amounts.
So in general, the duration of exposure would have been probably roughly similar to some of these studies. The intensity of the exposure could have been somewhat less, might have been somewhat less. We didn't have data to accurately characterize it in that population.

As I said, my examination of a small group of those individuals led me to believe that there was, indeed, a substantial prevalence of asbestos-related disease in that population, suggesting that they had a substantive exposure over time. That exposure over 40 years certainly could produce a substantial frequency of disease, which was manifest in the examinations that I conducted.

Q On Exhibit Number 8, is it fair to say based on your earlier testimony that the levels of asbestos -- of relative risk shown on the asbestos exposures based on what you said before, remained at that level once they -- once the individuals were no longer exposed to asbestos?

A I'm not sure what you're asking me.

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Q The level of relative risk individuals may have had if they fell within the applicable comparison cohorts that you've listed here on Exhibit 8, that is, the Cimino claimants, if they had a -- if they were in manufacturing, for example, and you somehow testified that they fit into one of these cohorts, or at least they had comparable exposures, in your opinion they fit, that level of asbestos exposure would remain constant, then, even after they -- the relative risk for lung cancer, rather, would remain constant even after they ceased their asbestos exposure?

A There is not a substantive body of data to define the answer to that question.

My understanding of the data that exists suggests that short-term expense -- intense exposure resulted in substantial increased relative risks, and those relative risks persisted after prolonged periods of absent exposure.

That in conjunction with the data from Selikoff studies showing that the relative risks for asbestos-exposed individuals defined -- declined with cessation of smoking was used to support the conclusions that I've offered that asbestos exposure relative risk doesn't appear to decline over time.

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To the extent that that conclusion is correct, in these populations of individuals one would expect that the relative risk would remain constant following cessation of asbestos exposure, and that that relative risk would be based on the accumulative asbestos exposure prior to the time that they stopped being exposed.

MR. SCHROEDER: Would you mark that one for me, please, ma'am?

Q The point of this chart, or one point of this chart was to show that if individuals quit smoking, that their relative risk for lung cancer would decrease based on the years since quit, that you've listed here; is that a fair statement, that that is one of the points on this chart?

A One of the points on this chart is that.

Q Okay, thank you.

A And it was intended to present that in a relative context to other relative risks.

Q Okay. All right. And you have -- I notice on

your insulators here, some of your insulators go as high as what appears to be 7 relative risk; is that a fair -- if you go over to the left, is that a fair conclusion to derive from that?

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A It would appear to be that high, yes.

Q What study was that; do you recall?

A I would have to go back and look to see exactly where that came from. It is likely that it may have been a subgroup of a study.

Q Did you make any assumptions on the declining risk aspect of this chart as to the number of cigarettes per day or pack years for these categories of 1 through 4, 5 through 9, or 10 plus?

A No. These were data taken from, I believe, the Surgeon General's reports on -- and it was either the Veteran's Study or the American Cancer Society CPS1 data. But I can't remember which I used to generate that figure.

I apologize for what was probably an obligation on your part to have waded through all of those graphs that were generated on the Cimino class. Some of them didn't make much sense.

Q Some of them did, and I would like to ask you about another one.

A Sure.

(Deposition Exhibit 10 marked.)

BY MR. SCHROEDER:

Q If you would take a look at what's marked as

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Exhibit 10, can you tell me, Dr. Burns, that that's another one of the -- I used the phrase "Cimino," and I'm probably not saying that correctly, but since I have lived with that, this is yet another Cimino chart, is it not, that you used?

A I believe so.

Q And this chart is --

A Well, let me be clear. It's another chart that I prepared in preparation for testimony.

Q Fair enough.

A I don't believe we ever used it.

Q Fair enough. But you prepared this chart based on your review of literature that relates to the topics noted on the chart; correct?

A That's right.

Q And you show us here, do you not, the relative risk of lung cancer for asbestos-exposed individuals based on certain studies; is that a fair statement?

A Certain studies and certain characteristics of the populations and different studies, yes.

Q And this is a relative risk of asbestos -- for asbestos exposure controlling for smoking; correct?

A I don't believe so, no.

Q Well, if you look over at the Selikoff group,

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you have a relative risk for those greater than 20 years exposure of seven. That certainly does not comport with the Selikoff conclusion.

A Yes. As I said, I would have to go back and look at the sources. I don't recall exactly what I used

at this point.
Q Okay. Well, looking at the two right-hand columns --
A This obviously is not a graph that ever went to final, given the number of errors that are contained on it.
Q Well, in fact, though, you testified to it in your deposition in the Cimino case, did you not?
A I may well have. I'm just saying that I didn't -- I don't recall the specific classifications that I used to look at these risks.
I don't believe that these risks were defined by risks being controlling for smoking.
I believe these are risks of asbestos-exposed populations in comparison to nonexposed populations without control for asbestos -- for smoking, I apologize.
Q The Selikoff study that you refer to on the right, is that the same study, whatever study it is, is
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it the same study just looking at people exposed less than 20 years and people more than 20 years?
A I'm not sure what you mean by the same study. It is the same -- I believe it is the same population. It is likely that it is a different analysis of that population. Probably the one that was contained in the New York, Annals of New York Academy of Sciences, where Dr. Selikoff described the effects of asbestos without defining the effect of smoking. In that same volume Dr. Hammond and Dr. Selikoff together examined the interactive effects. But both populations are presented.
I believe, and I would have to go back to check to be certain, that these data may come from the study of asbestos insulators, where the risks were categorized independent of their asbestos exposure. But I would have to go back and look.
Q Well, we know for a fact, do we not, that the Hammond '79 study established for people who had roughly 20 years or so of asbestos exposure a 1, 5, 10, 50 relationship, do we not?
A In the analysis to control for cigarette smoke.
Q Yes. And the 5 was the asbestos exposure, was it not?
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A The 5 was the asbestos exposure in those individuals who weren't exposed to cigarette smoke.
Q Correct. And isn't it true that for those individuals who did not smoke in that cohort who were exposed more than 20 years to asbestos exposure, that their relative risk for lung cancer was actually close to 7 or 8; that is, it was beyond 5?
A I don't recall data that specifically examined that population. And I would have to go back and look. I've not seen a breakout of the nonsmokers with longer exposure, at least that I recall. It may exist; I'm just not familiar with it by recall.
Q Are you aware of the study that you were referring to when you did chart Exhibit 10?
A Do I have it sitting here in front of me? No.
Q No. Do you have it available at your office or

someplace?
A I believe that is the Selikoff study published in the Annals of the New York Academy of Sciences. If that is the one, that is readily available.
Q Is that the one where Dr. Hammond is the lead author on, that gave us the 1, 5, 10, 50 relationship? Or are you talking about a different study?
A I thought I'd made that clear. But there are
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two descriptions of the asbestos insulator population presented in the Annals of the New York Academy of Sciences devoted to asbestos. Okay? That is the 1979 volume --
Q Right.
A -- that reflects that meeting. One is the study where Hammond is the first author.
Q Correct.
A That is the study that defines the interactions between smoking and asbestos exposure --
Q Are you referring to that one here?
A No. Where the 1, 5, 10, 50 numbers have been generated. There is also a study in that volume where asbestos insulators are examined as a group without characterizing them by their smoking status.
Q That's the study on death rates by Dr. -- I'm going to ask you, is that the one by Dr. Seidman that dealt with death rates?
A I believe that's a study by Dr. Selikoff as first author. There is also a study by Dr. Seidman on the same population that looks at some other aspect, which I think has to do with either future projections or some other characteristic that was of import.
But my recollection, and it is a recollection
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that is foggy with time now, is that the numbers from this may have come from Selikoff's article that he was the first author on rather than the Selikoff article that Hammond was the first author on.
(Deposition Exhibit 11 marked.)
BY MR. SCHROEDER:
Q Let me hand you what's been marked Exhibit 11, and ask you, is that another one of the charts from your Cimino work? Do you recognize it?
A I believe that it is, yes.
Q And this chart predicts, does it not, the percentage of persons exposed to, I take it, welding fumes; is that what this intended to show us based on ILO stratification?
A I believe -- I don't have a detailed recollection of what was done to generate this chart. I believe from the information provided on the chart, it is the percentage of individuals who were exposed to welding by their ILO classification.
Q Why would that have been important in the Cimino litigation?
A I'm not sure.
Q Did it relate to a question of whether there --
A I should, perhaps, tell you that many of these
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graphs were generated by a member of my staff over a

very short period of time under the urgency of trying to prepare for a deposition so that we would have some information to look at. He ran large numbers of graphs and combinations, many of which did -- had very little legitimate meaning in terms of how we would use them. This one potentially could show a relationship to welding exposure. It does not. But potentially it could have, I guess.

Q Well, you testified to this chart in particular under oath, did you not, in a deposition?

A I don't have a recall of the deposition sufficient to be able to tell you whether this was part of the deposition or not. It was likely that it was provided as part of the materials for the deposition. And if that likelihood was true, it's likely that we went through it.

Q All right. To the best of your recollection, then, what is this intended to depict?

A This is intended to depict, to the best of my recollection -- if it indeed is the data that I believe the title suggests that it is, and I don't know whether that's true, to the best of my recollection, this would be the percentage of individuals within each ILO

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classification who reported exposure to welding.

Q All right. Is it your understanding that persons exposed to welding have increased asbestos exposure because of the welding?

A No. It is my opinion, my reading of the literature, that people who are exposed to welding have an independent source of abnormalities on their X ray, and we were examining, I believe -- and this is a long time ago now, I believe we were examining whether that effect would be evident in this population.

Q So for example --

A The prevalence of -- well, go ahead.

Q I'm sorry. For example, then, on Exhibit 11, if you look at the ILO category of 1/0, would it be correct to read this chart as saying that for all of those in this population in the Cimino case who had a 1/0 ILO perfusion, noted that more than 60 percent of them, perhaps even close to 70 percent of them, were exposed to welding? Would that be a fair way to read the chart?

A This 1/0?

Q Yes.

A Well, it would be fair that they reported that on -- into this data set. If -- again given the caveats

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that this is something that is, indeed, that analysis.

Q Okay. Did you have any reason at the time in the Cimino case to doubt whether or not these persons were actually exposed to welding fumes?

A No.

Q You said in the 1985 Surgeon General's report that, "The abnormalities produced by smoking and those produced by asbestos exposure are usually quite different on chest X ray once the disease process is sufficiently advanced"?

A That's correct.

Q All right. And do you agree with that

statement today?
A I do.
Q And that you further stated that, "confusion about X ray diagnosis is --" I'm sorry, "in severe disease is unusual"?
A That's correct.
Q And do you stand by that today?
A Yes.
Q How do you define severe disease?
A Severe disease in general would be disease that has a perfusion of 2/2 or greater.
Q Okay.

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A Or alternatively, severe disease would be disease that has resulted in a 40 percent loss of lung function or greater.
Q And you said in that report that, "The radiographic changes associated with asbestos includes small irregular opacities which commonly begin as a radicular pattern in the lower lung fields and may progress to diffuse interstitial densities throughout the entire lung with reduced lung volumes."
Do you stand by that today?
A Yes.
Q You also said in that report that, "The abnormalities that have been reported with COPD, include overinflation, prominence of lung markings, tubular shadows, and in the presence of significant emphysema, oligemia," which is a deficiency of the amount of blood in the body, "and bullae"?
A Oligemia is actually deficiency of the amount of blood in the lung.
Q In the lung, okay. And bullae. Do you agree with that statement today?
A In general, yes, that's correct.
Q Would you agree with me that the studies upon which Dr. Nicholson relied in his report dealing with

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the question of whether smoking increased the prevalence of asbestosis among individuals exposed to asbestos measured not clinical asbestosis but rather solely the presence or absence of a parenchymal abnormality?
A In most cases, those studies examined the chest X ray abnormalities using the ILO classification schema which is not specific for asbestos, that's correct.
Q And most of those studies, virtually all of them, use as a lower threshold the ILO classification of -- not of 1/1, as stated in your testimony for clinical asbestosis, and as stated in the ATS document, but rather the lower threshold of 1/0; is that correct?
A He used the lower threshold commonly used in epidemiologic studies and that is supported by the people -- by my understanding, at least, of how the ILO classification was intended to be used, okay, of 1/0, so that you include all of those individuals who might have abnormalities on their chest X ray. That's the traditional epidemiologic approach using the ILO classification schema.
He used that. And I believe, at least as I recall his report, he conducted multiple approaches to that, some of which looked at the prevalence of a

reading greater than that and others that looked at the
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prevalence of readings by specific ILO classifications.
Q You would agree with me that the studies on parenchymal abnormalities use a lower threshold of 1/0, I think as you said, for epidemiological purposes, and cannot be equated with the diagnostic standard that you use for clinical asbestosis?
A I don't agree with that statement. I think that you're mischaracterizing a variety of things, and you're also mixing apples and oranges. The ILO classification schema was developed for purposes of epidemiologic evaluation of population. It's classification schema was intended to be very sensitive so that you could pick up early diseases in those populations. It is used for that purpose. It is used appropriately for that purpose with an ILO classification schema of 1/0 or greater. If one is making a clinical diagnosis on an X ray, in general one does not use the ILO classification schema; one uses evaluation of the X ray that says it's normal or abnormal and a description of the abnormality.
There are individuals who do that in conjunction with describing an ILO classification. In that setting, when one is making a clinical diagnosis, you're making the diagnosis based on being certain that
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there is an abnormality present. As I understand the ILO classification, that would translate to a 1/1.
So you're simply mixing apples and oranges in terms of how this schema would be applied.
MR. SCHROEDER: Would you mark that?
Q So is it fair to say based on that last answer that the standard used for the clinical diagnosis of asbestosis is different from the standard applied in -- for whatever reason, in the epidemiological studies that deal with the prevalence of asbestosis among smoking asbestos workers?
A I mean, you're asking me to compare things that are apples and oranges. An epidemiological study is intended to examine a population. And you have got a variety of different means by which you would classify that population. One of which would be its X ray classification.
In a diagnosis you are trying to define the presence or absence of disease for an individual. And it is uncommon to say, "I'm uncertain as to whether this test is normal or abnormal." And therefore, what one does is either make a decision that it is normal, after careful review, or that it is abnormal after careful review.
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So you are asking me to compare things that don't fit in the same context in terms of how the information would be used. I mean, I can't give you a reasonable answer for that reason.
MR. SCHROEDER: Would you mark that one, too?
Q The uncertainty you just referred to in your last answer is reflected by the diagnostic standard of 1/0; is that what you mean by that?

A My understanding of the meaning of the classification 1/0 is that you believe that an abnormality is present, but that you are uncertain that it is present.
Q Okay.
A It might not be present.
Q All right. Thank you.
A How about one last short break before we get on the freedom bird here?
MR. SCHROEDER: That would be fine. Off the record.
THE VIDEOGRAPHER: Off the record at 5:05 p.m.
(Recess.)
THE VIDEOGRAPHER: This marks the beginning of Videotape Number 5 of Volume 2 in the deposition of Dr. David Burns. We are back on the record at 5:14 p.m.
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BY MR. SCHROEDER:
Q Dr. Burns, would you agree that in looking at the lung function test of total lung capacity that smoking can actually cause total lung capacity to report a higher lung function than might actually be there because of the smoking effect? Do you follow that?
A I don't follow that. The disease process --
Q I'll withdraw the question. Let me rephrase it.
For a person who's exposed to both asbestos and smoking, isn't it true that the smoking component can actually cause the total lung capacity to appear to be normal when, in fact, it would have been reduced had there been no smoking?
A Yes. The disease effect produced by cigarette smoking results in a loss of elastic recoil and increased lung volumes. And that may offset disease in other parts of the lung that result in a reduction in lung volume. That is not -- it's careful -- it is important for you to understand that is not offsetting effects in the same parts of the lung. That is two different parts of the lung, both of which have been damaged in different ways.
Q But it's -- but if measured by total lung
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capacity, that measurement device can actually report normal when, in fact, an individual is jointly exposed; there could be impairment to lung function?
A That's correct.
Q And so if -- strike that.
And it's true, is it not, that in the absence of cigarette smoking, asbestos exposure itself does not result in clinically significant air flow obstruction of the sort seen with cigarette smoking?
A That is correct.
Q In the 1985 Surgeon General's report you said that, "If cigarette smoking contributes to the development of interstitial fibrosis in asbestos-exposed workers, the contribution is a minor one in comparison with the effect of asbestos dust exposure." Do you recall that?
A Yes. And I believe we've already answered that once before.
Q Did we cover that one before? And you agree

with that?
A I agree with that still. I haven't changed over the last 10 or 15 minutes.
Q Would you agree, Dr. Burns, that since all claimants who were compensated by the Manville Trust as
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a condition of their being eligible for compensation had to demonstrate prior asbestos exposure, that under those circumstances, asbestos was a substantial contributing factor to the development of their disease?
A Well, I've not reviewed the population in the Manville Trust. I'm not in a position to offer opinions about the specifics of who is or is not in the Manville Trust population. And therefore, I can't really respond to your answer. It is my understanding that there are some people in that Manville Trust who do not yet have any injury, but simply have asbestos exposure, and are in that database because they may subsequently develop an injury.
Q Which people are those?
A I'm not -- as I said, I'm not conversant with who is or is not in the Manville database. But it was my understanding that at least some of the individuals who are considered have applied for and received a -- some kind of certificate that says, "You have been exposed to asbestos. You are part of this group. You have no evidence of disease. But should you develop disease in the future, you will be compensated."
Now, I don't know for sure that they're part of that group. I haven't looked at the group. I'm not
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conversant with it. And therefore, I'm not in a position to offer you an opinion. So I don't know whether everybody in the group is diseased. I don't know the reality of what their asbestos exposures were. And I can't really answer your question.
Q Is this a group apart from the persons who have sought compensation for pleural plaques?
A I don't know.
Q All right. Well, you in the past have taken the position that pleural plaques are not a disease because there's no impairment; right?
A It is my opinion that pleural plaque itself does not produce a functional impairment consistent with producing functional limitation or disease.
Q All right. Would you agree that in an individual who has lung cancer, and they have more than one substantial contributing factor present, that is, both significant asbestos exposure and smoking history, that you as a physician cannot determine which factor in that individual actually caused the cancer?
A I think that that's not a fair characterization. I think a more fair characterization is that it would be my opinion that both made substantive contributions, and they would make
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substantive contributions in proportion to the dose, intensity and duration of exposure.
Q And since both made substantive contributions, both would bear some responsibility in the overall lung

cancer; is that your opinion?
A Yes. If there is a substantive asbestos exposure and a substantive smoking history and the presence of lung cancer, yes, it is my opinion that both make a contribution to the disease occurrence.
Q Very briefly -- I know Mr. Molster wants to ask some questions.
Let me ask you a couple of questions about the trust distribution plan. If you would pull that out. It's one of the exhibits marked as Number 5 that's in front of you, Dr. Burns. And I want to focus now on the disease in Categories 5 and 6.
A I don't want to get these confused for the court reporter here.
Q That's all right.
Did you find a copy? There it is on the very bottom.
Take a look at Category -- page 586 of the document which is page number 2. The Category 5 in the lower left column, would you agree with me that the
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claimant who has to file for Category 5 lung cancer has to demonstrate at least 15 years of heavy occupational of exposure to asbestos-containing materials in employment regularly requiring work in the immediate area of visible asbestos dust? Right?
A That is what it reads, yes.
Q And would you agree with me that for any person who has that type of heavy occupational exposure, that asbestos would be sufficient to create a substantial contributing factor to their lung cancer?
A I guess I'm a little confused by your language. It would be sufficient to make it a substantive cause of the occurrence of their lung cancer, yes.
Q And that's whether or not they also smoked; right?
A Whether or not they also smoked.
Q Okay. And would you agree with me under Category 6 of lung cancers, that there is no -- there is no requirement in Category 6 that you submit actual pathology of your lung cancer claim; isn't that correct?
A I'm not quite sure what you mean. You have to demonstrate medical report of the existence of a primary asbestos-related cancer of the lung.
Q Correct. And there's no requirement in there
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that you actually submit pathology from your biopsy, is there?
A How would you make that diagnosis otherwise?
Q I don't know. I'm asking you.
A The requirement to fulfill that would, with unusual exceptions, require some pathologic verification.
Q All right.
A You might not have to submit the slides, but you certainly would have to submit the medical evidence that that was true.
MR. SCHROEDER: Okay. I have a series of questions I wanted to ask you about a couple of other topics. But Mr. Molster has been sitting here patiently here for two days now -- well, for a day, to ask

questions. So I'm going to let him ask a few questions. I'm going to reserve my right for the record for the opportunity to continue my exam on the other issues, which is something we'll just take up later.

Thank you, Dr. Burns.

MR. MOLSTER: Thank you.

EXAMINATION

BY MR. MOLSTER:

Q Good afternoon, Dr. Burns. As I indicated

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earlier, my name is Charles Molster. I'm with Winston and Strawn. I represent Philip Morris.

Just to make sure that we're on the same wavelength, I have come all the way from Washington, D.C., to ask you some questions. As a result, I would be very appreciative if you would answer my questions and not volunteer additional information that is not responsive to my questions. I suggest to you that my questions for the most part are yes or no.

In response to an earlier question today regarding our discussion regarding epidemiological studies, you used the term, quote, "causal inference," end quote. Do you remember that?

A I don't remember it specifically. I commonly use that term.

Q And that term means an inference of causation; correct?

A That's correct.

Q That term does not mean scientific proof of causation, does it?

A In some contexts it does. But in general it means that you are implying causality.

Q And --

A You are making a statement of causality, yes.

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Q An inference of causality?

A An inference of causality that is often in a setting where you are stating that there is causality present.

Q But it's true, is it not, Doctor, that that's different than scientific proof of causation?

A It depends on the context of the statement.

Q Now, you've previously discussed today public health action; do you remember that?

A Yes.

Q And you've also talked about scientific proof today; correct?

A That's correct.

Q All right. It's true, is it not, that you agree that a different standard of causation supports the public health action as opposed to a standard of causation that rises to the level of scientific proof?

A There is an earlier --

Q That's really a yes or no question, Doctor. If you could give me a yes or no answer, that's what I'd like. If you refuse to, we'll deal with it, but I'm asking you --

A Are you constraining me to a yes or no answer?

Q What I would like is yes or no. If you want to

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explain it, explain it. But what I would like is a yes or no answer.
A Yes. There are different standards of proof. There is a threshold that one would use for being concerned. There's a threshold that would use for taking public health action.
Q Yes.
A And there's a threshold of scientific certainty.
Q Now, the threshold that you would use for taking public health action is lower; that is, a lower standard of causation than one would use to describe something as rising to the level of scientific proof; correct?
A That's correct.
Q All right. Now, specifically as to whether or not cigarette smoking causes disease, you agree that a lower standard of causation supports public health action as opposed to a standard of causation that rises to the level of scientific proof?
A No. I believe that both of those standards have been met.
Q That's not my question, Doctor. I didn't ask you whether or not they've been met or not met.

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Could you read my question back, please? If you would listen carefully to my question, Doctor, and only answer the question.
A I'm doing my best.
(Record read.)
THE WITNESS: That is phrased, as I understand it, in the present tense.
BY MR. MOLSTER:
Q No, I'm saying as a general matter, Doctor. It's not phrased as of today. I'm not asking you if there's a difference as of today. Would you read -- do you need it back again?
A I'm uncertain what your question is. Could you clarify --
Q We'll read it back again.
A Could you clarify --
Q We'll read it back. If there's a word you don't understand, you tell me that you don't understand it. But, Doctor, I've sat here all day long and seen you repeatedly answer questions, by saying "I don't understand. I'm not sure exactly what you're asking me." And the questions have been plain as day.
So we'll read this one back. If that's your response, fine, we'll deal with it.

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MR. GRUENLOH: Object --
MR. MOLSTER: If you've got an objection, object to form. No speaking objections, please.
MR. GRUENLOH: I object to that characterization.
MR. MOLSTER: Fine. Would you read it back?
(Record read.)
THE WITNESS: I believe the standard for both has been met.
MR. MOLSTER: Yeah, but that's not my question. I move to strike as nonresponsive. I'm going to

discontinue this line of questioning because of your answer. I'm going to go to the judge and ask for we come back to take your deposition, and I'm going to ask that it be at your expense or at the expense of plaintiff's counsel.

THE WITNESS: I have pointed out that I've asked you for clarification --

MR. GRUENLOH: Don't engage in this discourse, Dr. Burns.

BY MR. MOLSTER:

Q You're not an advertising expert, are you, Doctor?

MR. GRUENLOH: I'm sorry, hold on one second.

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Could you tell me what time it is? What time do we have on the record?

THE VIDEOGRAPHER: I show 5:28.

MR. GRUENLOH: Thank you.

BY MR. MOLSTER:

Q You're not an advertising expert, are you, Doctor?

A I have expertise in the application of tobacco advertising relative to smoking behavior.

Q You're not an advertising expert are you, Doctor?

A I have defined what my expertise is that relates to expertise in advertising. I am not a practitioner that develops advertising for use.

Q You're a medical doctor; correct?

A I'm a medical doctor.

Q You never worked in an advising agency, did you?

A I have never worked in an advertising agency.

Q You're not media expert, are you, Doctor?

A I have substantial expertise in the use of media for public health purposes. I'm not someone who has exclusive training or function in media.

Q Do you hold yourself out as a media expert?

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A I hold myself out as a media expert relative to the examination of the impact of media for tobacco control issues and have received grants for that purpose.

Q Do you hold yourself out as an advertising expert?

A I've told you my expertise relative to advertising.

Q My question is, do you hold yourself out as an advertising expert?

A I have received grants for purposes of examining advertising, and that is the basis of my expertise, the examination of those issues.

Q Do you hold yourself out as an advertising expert, Doctor?

MR. GRUENLOH: Objection; asked and answered.

THE WITNESS: I hold myself out as an expert in advertising relative --

BY MR. MOLSTER:

Q Have you --

A Please let me finish. Relative to the investigation of the impact of advertising on smoking

behavior and have received grants based on that expertise.

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Q Are you holding yourself out as an advertising expert in this case?

A I am holding myself out as an expert on the impact of tobacco advertising and counteradvertising on smoking behavior.

Q In this case?

A In this case. I don't know whether that expertise will be called for.

Q We have spent some time today talking about, you made reference both in your report and in your opinions today to epidemiological studies; is that correct?

A I have used that term.

Q Epidemiological studies use statistics to try to investigate the lengths between diseases and the factors which may cause those diseases?

A All studies scientifically, in general, use statistics. Epidemiologic studies use epidemiologic approaches and then apply statistics to the data that they generate.

Q Can you give a yes or no? Are you unwilling to give a yes or no answer to my question?

A I think your question mischaracterized epidemiologic studies. I'm simply trying to be clear.

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Q A relative risk is the statistical measure which expresses the findings of an epidemiologic study; isn't that true, Doctor?

A No. It is the epidemiologic measure that compares the death rates in two populations.

Q Now, Doctor, you agree, as I understand it, that in your view, the lung cancer risks caused by cigarette smoking are proportional to the intensity and duration of the exposure to cigarette smoke; is that correct?

A That's correct.

Q And is it also your view, Doctor, that lung cancer risks also increase directly in proportion to the intensity and duration of asbestos exposure?

A That is also correct.

Q Okay. Now, is it -- it is also true, then, isn't it, Doctor, that any relative risk model regarding the health effects of smoking and tobacco would require adequate consideration of at least the following information regarding the individuals that make up the population being modeled: Number 1, the length of time of exposure to asbestos. Number 2, the level of exposure to asbestos during that length of time. Number 3, the length of time that the individual smoked

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cigarettes. And Number 4, the amount of smoking during that time that the individual smoked cigarettes?

MR. GRUENLOH: Object to the form of the question.

THE WITNESS: The answer to the question is no. It is possible to mathematically model populations based on data more limited than that you have available in

your list.
MR. GRUENLOH: Counsel, yesterday when you weren't here we reached a stipulation that we were only going to go until 5:30 today. I'm going to hold you to that. I apologize that your counsel didn't give you enough time, as I see that you have more questions.
MR. MOLSTER: I've got a lot more questions.
MR. GRUENLOH: We're going to go ahead and conclude the deposition.
MR. MOLSTER: You can get up --
MR. SCHROEDER: Just for the record, the stipulation was that we wouldn't go past 5:30 based on his time. But we didn't stipulate that the deposition would be concluded. And so we have no agreement we would finish the deposition.
MR. GRUENLOH: Let me also put a statement on the record at this point. In the interest of time and
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efficacy I've tried to limit my objections during the course of this deposition to a minimum, despite the fact that defense counsel have often asked vague questions and asked those same questions over and over again to the point that it's bordered on harassment.
Prior to today the defendants postponed this deposition with little or no reason at least three or four times. And the correspondence between counsel bears that out. It is our position that defendants have spent much of their deposition time on issues which have been brought in Dr. Burns' prior depositions and testimony, including issues of addiction, low yield cigarettes, and the processes through which Surgeon General and scientific field reaches consensus on the issues of tobacco and disease causation.
So again I object to the scope of the defendants' questions and defendants' conduct in this deposition. Specifically Mr. Bernick's practice of continually interrupting the witness and his attempts to intimidate this witness.
I think that the transcript of this proceeding, when it's looked at in its entirety, will bear this out and will reflect that this objection -- all of these objections are well grounded and based in fact.
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In light of these facts, I object to any suggestion that the defendants be given any more time in addition to the two days that they have already had to examine Dr. Burns.
MR. SCHROEDER: I'm not going to spend any more time arguing on the record. We can argue with the judge.
MR. GRUENLOH: Sounds good.
MR. SCHROEDER: Our position is the deposition is not over, and we'll leave with it that.
Thank you, Dr. Burns.
MR. MOLSTER: I have more questions. You can get up and leave if you want to get up and leave. But let's just stay on the record. I'm going to put a couple more questions on the record.
MR. GRUENLOH: You're going to ask questions without the witness?
MR. MOLSTER: I'm just going to put on the

record some of the topics that I would go through if you weren't getting up and leaving. You can get up and leave. I mean, I can't stop you from doing that.

MR. GRUENLOH: You know, Counsel, you weren't here yesterday when we reached that stipulation. You came in halfway through the day today. You're not even

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aware of the conduct that went on yesterday.

MR. MOLSTER: I didn't come in halfway through the day today.

MR. GRUENLOH: Yes, you did.

MR. MOLSTER: No, that's not true. I was here before the deposition started.

MR. GRUENLOH: If you want to --

MR. MOLSTER: Then I went to do a conference call with Judge Gold. So that's a direct misrepresentation you just made on the record about me coming in halfway through the day today.

MR. GRUENLOH: I don't know what time you came in --

MR. MOLSTER: I came in as soon as we got off the phone with Magistrate Gold.

MR. GRUENLOH: Well, it was after the deposition had started. And you certainly weren't here yesterday.

MR. MOLSTER: Let the record reflect that obviously counsel and the witness are leaving the room over our objection, and we will take it up with the court. Okay. Thank you.

MR. GRUENLOH: In accordance with the stipulation we reached.

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THE VIDEOGRAPHER: This concludes Volume 2 in the videotape deposition of Dr. David Burns. The number of videotapes used was five. We are going off the record at 5:36 p.m.

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I, DAVID M. BURNS, M.D., do hereby declare under penalty of perjury that I have read the foregoing transcript; that I have made such corrections as noted herein, in ink, initialed by me, or attached hereto; that my testimony as contained herein, as corrected, is true and correct.

EXECUTED this _____ day of _____, 2000, at _____, _____.

(City) (State)

_____

DAVID M. BURNS, M.D.

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STATE OF CALIFORNIA)
: ss
COUNTY OF SAN DIEGO)

I, the undersigned, a Certified Shorthand Reporter of the State of California, do hereby certify:

That the foregoing proceedings were taken before me at the time and place herein set forth; that any witnesses in the foregoing proceedings, prior to testifying, were placed under oath; that a verbatim record of the proceedings was made by me using machine shorthand which was thereafter transcribed under my direction; further, that the foregoing is an accurate transcription thereof.

I further certify that I am neither financially interested in the action nor a relative or employee of any attorney of any of the parties.

IN WITNESS WHEREOF, I have this date subscribed my name.

Dated: _____

_____

RENEE KELCH

CSR No. 5063

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